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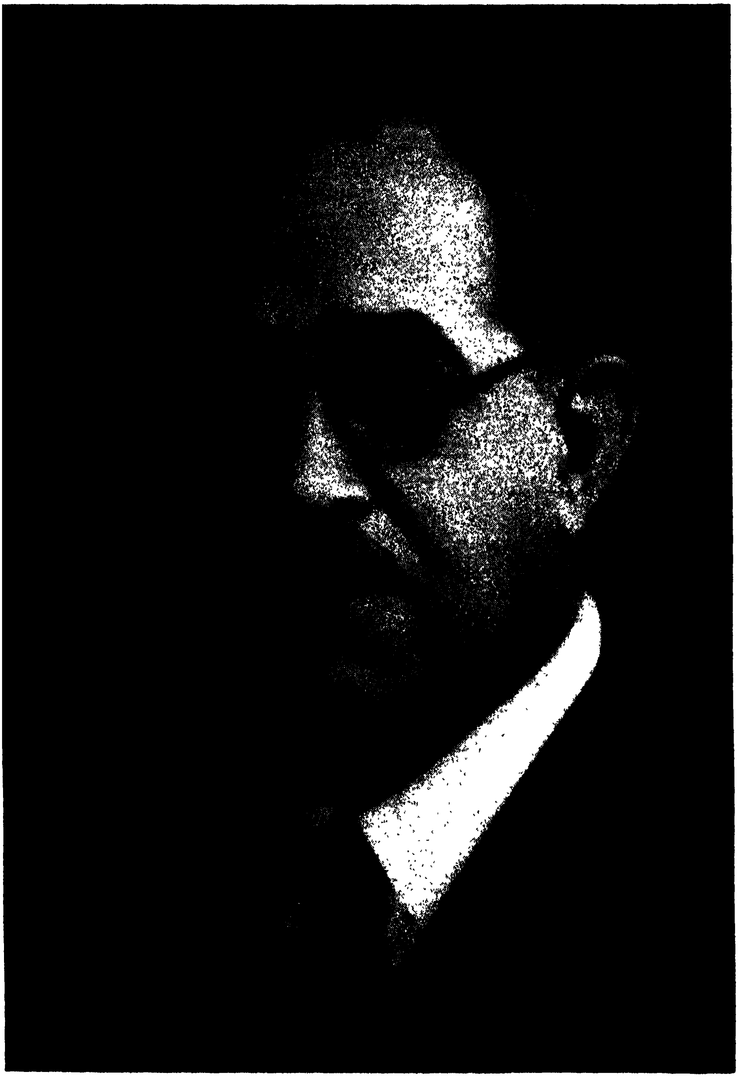
Carl Alsberg

SCIENTIST AT LARGE



Carl Alsberg

SCIENTIST AT LARGE

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STANFORD UNIVERSITY PRESS
Stanford, California

STANFORD UNIVERSITY PRESS
STANFORD, CALIFORNIA

LONDON: GEOFFREY CUMBERLEGE
OXFORD UNIVERSITY PRESS

THE BAKER AND TAYLOR COMPANY
55 FIFTH AVENUE, NEW YORK 3

HENRY M. SNYDER & COMPANY
440 FOURTH AVENUE, NEW YORK 16

W. S. HALL & COMPANY
457 MADISON AVENUE, NEW YORK 22

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OF THE LELAND STANFORD JUNIOR UNIVERSITY

PRINTED AND BOUND IN THE UNITED STATES
OF AMERICA BY STANFORD UNIVERSITY PRESS

Foreword

Carl Alsberg came to the end of a richly productive life on October 31, 1940, in his sixty-fourth year, after an illness that developed rapidly on a train journey from New York to California in the preceding week. Other natural scientists and social scientists have achieved greater fame, but few have left so deep and constructive an impression on a circle so far-reaching in intellectual interests and in geographical distribution. His published works, though numerous and significant, constitute the smaller part of his pervasive contributions. In introducing him on Commencement Day at Reed College in 1938, President Dexter Keezer said in part:

> It can be said with an extraordinary degree of truthfulness that Dr. Alsberg has lived the curriculum of a liberal college. And, more important, he has done so with great distinction in every field he has entered.

Soon after his death, groups of his friends at Berkeley and Stanford conferred about possible ways of paying tribute to him in some enduring fashion. We felt the urge to share, not only with thousands who had prized their association with him, but also with their counterparts now and in decades to come, a more intimate knowledge of this extraordinary man than any one individual possessed. Accordingly, after rejecting proposals of other types, the general plan of this little book was formulated and endorsed by friends at the two universities where he had last served.

As I wrote to the invited contributors in April 1941, on behalf of the originating group, the book was to be designed "not only for Carl's many friends but also for scientists, scholars, administrators, and their potential successors, to many of whom his life and career seem to us to have a distinctive message." Within a short time, commitments for writing six chapters were made, and it was hoped that the book could be published in 1942. The first three chapters were indeed written in 1941, but the exigencies of war, and a change in the group of contributors necessitated by it, delayed completion of the fourth and fifth.

A sixth chapter was planned to "summarize the significance of Alsberg's life and career, with special emphasis on the evolution of a natural scientist into a leader in the social sciences and international relations." Frederick P. Keppel, long president of Carnegie Corporation of New York and an intimate friend of Alsberg's since college days at Columbia, wrote me in May 1941: "I'll be very proud indeed to write the last chapter." He was enthusiastic about the whole enterprise, and early in 1942 wrote Kroeber: "I think the book will be an important document entirely apart from our desire to honor the memory of an old friend." Before Keppel's untimely death in September 1943, he had made little progress toward fulfilling his commitment. When the other chapters were all in draft, however, it seemed that no one but Keppel could have added substantially, except in further detail, to what was already written.

Each of the contributors to this volume had a special relationship to Carl Alsberg. Kroeber was a lifelong friend, and Alsberg wrote the personal reminiscences of him for the volume of *Essays in Anthropology* published in Kroeber's honor in 1936. Van Slyke was a scientific friend who was most closely in touch with

Alsberg during the years 1908–12. Linton was his valued administrative assistant in the Bureau of Chemistry, 1912–21. Calkins was with Alsberg at the Food Research Institute during most of the years 1925–32, thereafter a member of various committees with him, and his Berkeley colleague in 1937–40. Condliffe was intimately associated with Alsberg in the work of the Institute of Pacific Relations beginning in 1927.

In addition to their five contributions it seemed appropriate to include three of Alsberg's own papers and a classified bibliography of his published works. This last was prepared by his secretary for twelve years at Stanford, Ruth Lee Young.

Other friends and former associates have anonymously aided in the preparation and revision of the contributed chapters, and many more would have been glad to do so.

Stanford University Press, under the directorship of Donald P. Bean, welcomed the opportunity to publish a book about a man who had contributed signally to Stanford's evolution and to the larger world through his service here. Carnegie Corporation of New York made a grant to assure its publication.

My own deep affection and high esteem for Carl Alsberg are implicit in the guidance that I have eagerly given to the project through its various stages. I have regretted the lapse of time before it could be carried to completion, chiefly because each year has taken its toll of those who would have read the book with keenest appreciation. Yet eight years is a short time in the lives of men, and the subject is almost timeless.

JOSEPH S. DAVIS

FOOD RESEARCH INSTITUTE
STANFORD UNIVERSITY
April 1948

Table of Contents

FOREWORD, vii
Joseph S. Davis

THE MAKING OF THE MAN, 3
Alfred L. Kroeber

WORK IN THE NATURAL SCIENCES, 23
Donald D. Van Slyke

CHIEF OF THE BUREAU OF CHEMISTRY, 35
Fred B. Linton

UNIVERSITY PROFESSOR AND ADMINISTRATOR, 65
Robert D. Calkins

SOCIAL SCIENTIST BEYOND THE UNIVERSITY, 101
John B. Condliffe

PROGRESS IN CHEMISTRY AND THE THEORY OF POPULATION, 122
Carl L. Alsberg

WHAT THE SOCIAL SCIENTIST CAN LEARN FROM THE NATURAL SCIENTIST, 136
Carl L. Alsberg

COMMENCEMENT ADDRESS AT REED COLLEGE, 142
Carl L. Alsberg

ALSBERG'S PUBLISHED WORKS
A CLASSIFIED BIBLIOGRAPHY, 155

INDEX, 175

Carl Alsberg

SCIENTIST AT LARGE

The Making of the Man

By

ALFRED L. KROEBER*

Carl Lucas Alsberg was born in New York City, at 98 Second Avenue, on April 2, 1877, of intellectually liberal, middle-class German-Jewish stock. Their influence on his personality and career, though veiled, was as profound as his own experiences within American life.

The family name derives from his great-grandfather, Lucas Samuel, of Volkmarsen in the grand-duchy of Hessen, who, in conformity with new laws of Napoleonic days, took as his civil name the designation of the Als Berg, a near-by hill crowned with a ruined castle. He was a merchant and owner of a dye works—the first premonition of the chemists of later generations. One of his sons, an older Carl Alsberg, a wine dealer in Volkmarsen, married Johanna Baruch, of a family which was to enter doubly into Carl Lucas Alsberg's ancestry.

The Baruchs were from Hörringhausen in Hessen, and a less stable if more adventurous line than the Alsbergs. In 1783, Meyer Baruch bought himself a house in Arolsen, a somewhat larger town of the region. He was succeeded by his son Solomon, a very un-Jewish character for his day, a giant in frame, a hunter and drinker, twice mayor, and, according to tradition, an associate of the local aristocracy. It was his daughter that the older Carl Alsberg married, and in 1851 moved with her and their twelve children (of whom ten lived to become adult) to the house in which she had been born.

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* Emeritus Professor of Anthropology, University of California, Berkeley, California; past president of American Anthropological Association, of American Folk Lore Society, and of Linguistic Society of America; Huxley Medal, 1945; Viking Medal, 1947.

Into this family Meinhard, the future father of our Alsberg, was born in 1842. He was made to get up at six and study Hebrew before school; and to her ripe old age his mother would have no pork on the table, though she connived at the servants' treating the visiting grandchildren from America to Westphalian ham. The bourgeois liberalism of mid-nineteenth-century Germany was spreading, however, and her own children grew up free-thinking and interdenominational. One sister became a pianist, another a governess; two of the boys took a Ph.D. in chemistry. One of these was Meinhard, who studied at Göttingen under Wöhler, discoverer of aluminum; at Marburg under Bunsen; and at Jena under Genther, to whom he became an assistant. He specialized in organic chemistry; his dissertation was on valeraldehyde. For a time he was *Privatdozent* in chemistry at Jena.

At the age of twenty-three Meinhard Alsberg emigrated to America, either because of lack of prospects for an academic career, or following his maternal uncle Meyer Baruch, the second, whose stepdaughter he had met and later married. In New York he was with the chemical firm of Anspacher Brothers, for whom he developed a process of manufacturing the deep-red dyes known as "lakes." He was also chief chemist for New York City's first Board of Health, organized by Charles F. Chandler, under whom he had earlier served as assistant in chemistry at the School of Mines, Columbia College, and under whom his son Carl was later to study chemistry at Columbia. Finally he started his own firm, M. Alsberg and Company, backed financially chiefly by his friend Isador Schloss, with a factory in Brooklyn. There he specialized in producing lakes and Paris green. He was one of the founders, and in 1878 secretary, of the New York Chemical Society, from which the American Chemical Society developed. Thus the career background for his future son was built up.

Meinhard's marriage to his cousin Bertha brings us back to the Baruchs whom we have already encountered. The picturesque Solomon left a no less picturesque son, Meyer Baruch, who studied for the rabbinate, became an atheist and quit, and finally ended up in New York as a successful homeopathic practitioner, defying the authorities when they pressed for details as to his medical degree. He was militant in other ways: tradition has it that he made a point of going to the synagogue on the Day of Atonement, defiantly chewing candy and reading a yellow-backed novel in a rear pew. His wife was also an emigrée, Babette Fuhrman, of peasant stock, born near Kaiserslautern in the Palatinate. She had come to America at the age of ten with most of her village when failure of crops ruined them. Babette had four children, two by a previous marriage, and two by Meyer Baruch. Of these children the second, Bertha, married her step-cousin Meinhard Alsberg, and had Carl Lucas as her first-born. Bertha was conditioned to the vivacity, energy, and imagination of the Baruchs; her husband had the quiet tenacity and character of the Alsbergs. The couple met for the first time when Babette took her four half-grown children for a visit to Germany, much like Bertha's visit with her four a generation later. Bertha, of course, was born in New York and married Meinhard there. Two of her brothers were physicians.

This was the setting in which Carl Alsberg was born in 1877, and after him Julius, Elsa, and Henry, at about two-year intervals. Soon after his birth, the family moved to Seventy-fourth Street near Lexington Avenue. Circumstances were comfortable without being affluent. Making a living was taken for granted, without becoming a preoccupation; intellectual, public, and even aesthetic interests were in the foreground, and dominated conversation and social relations. The language of the household was wholly German until English crowded in with the children returning from school; they grew up completely bilingual. At least once in one's youth one expected to see the Germany of his ancestors and older relatives. Carl did this at the age of seventeen, and, as the man of the family, learned responsibility about trains, hotels, and money, while his father remained with the chemical plant for the summer. Germans who visited America were more likely to be scientists, engineers, or artists than relatives or businessmen. Through the years a stream of these flowed through the Alsberg home, with the result that German universities, medicine, and science became something familiar and taken for granted by young Carl, and in no sense an alien model.

The life, in which I also participated, was called German-American, or more often Deutsch-Amerikanisch, but was in no sense felt as hyphenated. As regards all public affairs we were wholly American. I came nearer falling out with Carl than at any other time that I can remember when at the age of seven and eight we walked up Madison Avenue from Fifty-fourth to Seventy-fourth Street arguing the Blaine-Cleveland campaign. Carl's father held that virtue resided in the only patriotic party, whereas mine put character of the candidate first—though I suspect his Mugwumpery was a bit tainted by previous Democratic predilections. And we were all intensely anti-Tammany. On the other hand, none of us seemed ever to have personal or familial relations with Americans of Anglo-Saxon origin until we were well along in school. Our fathers dealt with them as customers and our mothers bought groceries and milk from them; but somehow we scarcely got to associating with them. Not that there was a barrier; but life was full and preoccupied without them. At twelve I was sent for a year, for reasons of health, to a boarding-school in rural Connecticut, and immediately felt completely at home with the New England Butlers and Andrewses. Carl's experience in high school—collegiate institutes they were called then (New York City was still without a public high school)—was the same.

In one respect, his and my experiences contrasted. My people, being Gentile, had no hesitation in sending me to the Sachs School—four-fifths of whose pupils were Jewish—paradoxical as that may sound today. Carl, however, from about the age of eight, attended the Harlem Collegiate Institute, later called the Mount Morris School. Its principals in his day were Jarinot, a Frenchman, and then Frank Lyman, an extraordinarily able teacher, who put him through his college-entrance Greek with flying colors in one year.

The relation of Gentile and Jew of German extraction in New York of the 'seventies, the 'eighties, and even the 'nineties was really quite extraordinary, at least for the middle and professional classes. One knew the difference, but assumed that it did not matter. There was a solid ghetto in Manhattan and an incipient one in the Bronx; and Avenue A was a "Little Germany" which supported the Thalia, the Irving Place, and for a while another German theater. In the upper economic levels, Jewish brokers and bankers evidently maintained a somewhat separatist social segment of their own. But the bourgeoisie that had crossed the ocean neither from direct need or trouble, nor from desire for increase of wealth, but for new or better opportunity, came ingrainedly and almost fanatically liberal. The roots of their conviction lay in the French Revolution, though their Germanic provincialism rendered them unaware of this fact; and they were instinctively both anti-Jacobin and anti-Napoleonic. But creed and race were to be wiped out, or at least ignored.

None of the German families I knew were church members. None of the Jewish families went to synagogue—a few perhaps on the Day of Atonement, in satisfaction of their elders or the memory of these. But they took for granted that one believed nothing in religion. Ex-Catholic and ex-Protestant Germans, being in nearly equal numbers, probably contributed to the disintegration of barriers. There was less intermarriage than might have been expected—the day of Jews and Jewesses wanting Gentile spouses had not yet arrived; but any avowed shrinking from a hybrid union would have been regarded as benighted and intolerant. Hans Zinsser's account of his boyhood gives the same picture, with a different nuance—due to his parents having perhaps held a slightly higher status in Germany or attained a more successful economic one in the United States. The Zinssers associated with Jews, but got involved with them less. They professed and tried to look up to the same principles, but there was some consciousness of Semitism. It is the base of Hans Zinsser's autobiographic ambivalence which evidently worried him a bit to the end. His father and mine planned and built adjacent summer homes on Sheepshead Bay, and for ten years I played chiefly with Hans in summers and in winters with Carl, without consciousness of difference. In fact, when Carl came to visit, he and I and Hans regularly ganged together against Hans's older brother and my younger.

In one minor way the varying antecedents did show. In Carl's home, two subjects were not discussed: sex and religion. In mine, only one was taboo: sex. Carl's ebullient mother was religiously indifferent and uninterested. But his father, raised under a mother who ate only kosher foods and made him learn the Torah, was still in revolt, and his agnosticism was aggressive. The children should live completely free and know nothing of trammels. If there was a God, or they wanted one, let them find Him for themselves when they had grown up. With us, it was my extravert and self-made father whom religion bored. One of my mother's parents was Protestant, the other Catholic: on both sides there were apostasies from one faith to the other for generations back. She was baptized a Catholic: therefore I was baptized Lutheran—and my younger brother and sisters left unbaptized, as befitted enlightenment. But the religious heritage was not so easily abolished: my mother joined Ethical Culture, a society founded by an agnostic rabbi who came to lead a part-Gentile congregation; and she instigated me to sample Sunday services there, and in Unitarian and liberal Congregational churches. Carl escaped these vacillations. He was encouraged to read Huxley's scientific essays; I, his controversies with the Bishop of Peterborough. Of course, I sided with Huxley, but had a sneaking feeling that the manners were with the Bishop.

I speak about myself because this milieu was joint.

Even the slight differences emphasize its essential integrality. I speak also with a certain nostalgia, because, with all its naïve idealism, this way of living in which we were reared, impossible as it would be to resuscitate it, does represent a solution, however transient and provincial, of a problem that is still agitating the world. And being born into this set of habits and ideals profoundly affected all Carl Alsberg's career. It gave him for life the rare and valuable attitude of being every man's inherent equal and no one's superior; a genuine ease and warmth, supplemented by his native kindliness, in relations with people; an ability to subordinate himself to the majority, to authority, or to circumstances, without suspending his independence of private judgment; and an even more outstanding faculty of directing others with their full loyalty and approval.

Carl neither evaded nor obtruded the fact of his origin. He never capitalized on it, nor let it make him feel balked. He succeeded in making the world take him as its full citizen because he unwaveringly felt himself such a citizen. There was no issue for him, so he never had to fight it.

One other factor was of permanent influence: the unalloyed respect which this environment had for learning and the arts, and the premium which it put upon their cultivation. They were not something compartmented, a segregated specialty of the few, but things of daily experience, of constant contact. Not everyone became a scholar, an artist, a practitioner of a learned profession; but everyone lived in an atmosphere of association with them. The most gifted slid easily into the status of such a career; the others maintained the association and attitude. This value for enlightenment may have been slightly reinforced in some Jews by a transmuted carry-over of the fetish of rabbinical learning. Essentially, however, it was the carry-over of the Europe of Voltaire, which the founders of America of necessity largely escaped, in spite of the genius exception of Franklin. The form which molded Carl Alsberg was its mid-nineteenth-century German facies: the mellow golden sunset of the German civilization of Kant and Goethe, translated to a late afterglow among congenial and harmonious American institutions, while the shadows of rigor were imperceptibly creeping up in the homeland. Carl Schurz was long a symbol of this cultural injection into our American commonwealth. Wendell Willkie is an equally significant symbol. One marks the conscious adaptation, the other the completion of the absorption. Meinhard Alsberg belonged almost to the generation of Schurz, Carl Alsberg almost to that of Willkie.

How strong these influences were appears from comparison with that most vivid and skillfully accurate sociological reminiscence, Clarence Day's *Life with Father*. The New York pictured is the same, except that the economic-social level is a shade higher: office on Wall Street instead of Brooklyn, home in the Forties and Madison Avenue instead of the Seventies near Lexington. There are the identical patriarchal authority, looseness of the religious yoke, fundamentally puritan self-respect, sub rosa juvenile interest in professional athletics, and all the rest. Only the intellectual and aesthetic suffusion of life is lacking. Day's Father read Gibbon's *Decline and Fall of the Roman Empire* while he relaxed on the couch and twisted his flexible toes; there is nothing to show that he ever discussed Gibbon or Rome, and we may be sure that few musicians or scientists penetrated a second time to his dinner table.

The problem of education loomed large in the Alsberg home. The New York public schools in the days of Tweed and Croker evidently maintained a kind of harsh, sodden efficiency, but they were certainly untinged with enlightenment. Carl learned the beginnings of his three R's at home from his mother. Then a tutor was found for him in the late afternoon hours. This was Dr. Bamberger, principal of what later became the Ethical Culture School, then known as the Workingmen's School on West Fifty-fourth Street, open only to children of the underprivileged. It was in substance the progressive school of its day, based on the principles that science began with observation and that manual training should precede the verbal and abstract. So Carl was taught a bit of clay modeling, charcoal drawing, and a sort of carpentering or carving, along with his numbers, and stories of the Trojan War were interspersed with his reading until he could read them himself. Bamberger was unmanual himself, and the shop work probably was more effective in satisfying parents' principles than in developing real dexterity; but it was a genuine endeavor. Stimulation of scientific curiosity was an important part of the program, and in this Bamberger was expert. The Workingmen's School possessed a miniature planetarium in which children could crank three planets the size of apples and plums to revolve creakingly around a yellow melon sun. Mud pies were made and left over night to illustrate the folding of the "drying" earth's crust into mountain ranges. My parents having the same predilection, Bamberger once suggested that he now and then merge his Alsberg and Kroeber hours into a science stroll in Central Park—"nature walks" they called them later; and that is how Carl and I, aged six and seven, became friends. He was probably the taller and certainly the heavier then, big-framed and large-footed, already deep-voiced, quiet, slow-moving, and full of friendliness for the world. A year later he was allowed to spend an hour or two, a few days a week, at the Workingmen's School. There he learned to dissect the nerve cords out of etherized crayfish and glue their chitin segments in position on cardboard. The details were sometimes ridiculous in their naïve imbalance for children, but the program achieved much of the stimulation at which it aimed.

At home, as he grew older, Carl was provided with scientific apparatus for childish experiments rather than with mechanical toys. These the boys were perhaps expected to build for themselves; but as there was only a lukewarm premium of praise for dexterity of mechanical accomplishment, new experiments usually took the place of constructions. Manually, Carl became only competent with apparatus. He was no technician; what he brought into the laboratory were ideas of problems.

Meinhard Alsberg was something of a connoisseur of etchings, and owned a good collection which Carl early learned to appraise. To the end of his life Carl remained most interested in pictures, of all aesthetic products. Literary preoccupations were to the fore in his college years, when he was one of a group of five which founded the magazine *Morningside*, named in prospect of impending removal from Forty-ninth Street. It was in the nature of a secession from the staid *Literary Monthly*, or reaction against it. Somewhere in its files, if any are in existence, is a ballad by Carl, on King Olaf, I believe. His brother remembers it as in the manner of Erlkönig, I in that of Heine; but it had genuine ring. Later, *Atta Troll* was his favorite work by Heine; to most of us this particular appreciation comes only with full maturity. Carl's mother sang and could render Schubert with fervor; a brother mastered the cello; he himself conscientiously took piano lessons, but quit early. Typical of his gift for organization is the fact that he found his greatest musical stimulation when his teacher set forth to him the outlines of the symphonic form. Carl restated this all with animation at the dinner table, and was somewhat dashed when an innocent younger brother asked whether he could now write a symphony.

To the Alsberg children, Meinhard Alsberg remained in memory as a stern father, against whose justice their jolly and easy-going mother often protected them. My recollection of him is milder. He was a gaunt, bearded man with flapping trousers, who loved to lounge genially around the house in slippers and a very old coat; taciturn, but with sly jokes for the kids, or on them. He was certainly illimitably tolerant of their fun. A visit to the Alsbergs meant a roughhouse such as was never permitted at our home, with or without visitors; battles with brooms up and down the stairs, water fights in the attic, and a general piling on top of one another of the eight of us wherever we chose. Father Alsberg only smiled indulgently, and Mother Alsberg rejoiced. The furniture was as shabby as comfortable, with no heirlooms or valuables around to be broken, and absolutely no pretense of show. Food was bountiful, and if it was a party, charlotte russes were pressed on us by Mother Bertha until we gagged. She loved seeing children eat. She herself would, during an evening's conversation, eat through a bowl of apples; her husband was the abstemious type. Carl inherited her generous appetite. He learned early the value of a good meal in dissipating the wear of life, and to the end he retained his gusto for well-cooked food. One minor item impressed me as one of the first of the injustices of which the world is full. In our house the rule was that between courses hands must be on the tablecloth, so that any visible idling or toying could be checked by reprimand. With the Alsbergs tradition was contrary: hands not holding fork or spoon had to disappear into one's lap. A hand left on the table was always likely to be smacked by father's sudden long reach; whereupon his eye would twinkle in a studiedly expressionless face—he had scored a point in a little game.

Carl passed his entrance examinations to Columbia when he was two months older than fifteen. He was runner-up for the competitive alumni scholarship. At graduation, he just failed of making Phi Beta Kappa, being of the two or three considered but not chosen. He was never a grind, and life was full of things outside the curriculum. There was no varsity football at Columbia then—even rowing had been allowed to lapse and was not revived until his junior year. Carl went out for freshman football and track, and was able to do a tolerable half-mile and a mediocre pole vault. He was not enough of a sprinter for the one, too lumbering for the other. He did place second in the eight-eighty in one dual meet, I believe, and once won from handicap.

The first half of the college course was pretty rigorously confined to the humanities: only chemistry and physics were allowed as electives. The last two years, however, were as good as wide open. Carl did about as well in the humanities as in the sciences. But as junior and senior he came under the influence of Edmund Beecher Wilson, for whom he did an experimental thesis on the embryology of the belatedly primitive garpike.*

Summers were spent as a boy on the Sound side of Long Island, then in an overlooked corner of the Berkshires which had grown back to wilderness. During college years, there were summer back-pack trips about Elk Lake and the peaks of the Adirondacks. But parts of the college vacations were spent in acquiring additional training in chemistry in the laboratory of Endermann and Saarbach, general analytical chemists, and in his father's factory laboratory.

On graduating from Columbia in 1896, Carl entered its Medical School, the famous "P. and S." on Fifty-ninth Street. A year later his father died suddenly of a wrongly diagnosed illness while on a brief vacation in the Adirondacks. Relatives and business friends advised that Carl should take over the chemical factory. He was reluctantly willing and, though only twenty, would probably have succeeded in retrieving the whole investment and increasing it, if the judgment and managerial capacity which he showed later in noncommercial enterprises are a touchstone. Finally, however, his mother declared that Carl's career was more important than additional income, and he returned to P. and S.

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* Carl's fondness for small discussion groups was lifelong. As a boy he helped to form a little "Humboldt Scientific Society," the chief object of which was to go with other boy members on collecting excursions and to hold discussions on winter evenings; this lasted into his sophomore year in college. The next year he belonged to a Bancroft Literary Society ("BLS") that met weekly at the homes of members. Some postcards reveal his activity in this club in the spring of 1895. By appointment, he read an essay on "The Object of Life" on May 12 and another on "Pitt" on May 26. On May 19, he was one of the negative speakers in a debate on the question: "Resolved, That Realism Conveys a Greater Moral Lesson Than Idealism." At Harvard he was a member of a similar, maturer group, the Bowditch Club. During most of his Stanford years he belonged to the "Saturday Morning Club," a group of twelve to fifteen Stanford and Palo Alto men who met twice a month on Friday evenings. His nine-year membership in the larger Chit-Chat Club of San Francisco lasted until his death. His last paper for this club, in 1939, was on "The Breakdown of the American Cabinet System." He found stimulating and enriching such discussions with people of different types, training, and interests, and they entered significantly into the making of the man.—Ed.

On becoming an M.D. at twenty-three and simultaneously getting an A.M., he waived an internship and decided to study in Germany, not for any further degree but in order to do research fundamental to medicine, especially in chemistry; the term biochemistry was only beginning to be used. In this decision Isaac Adler had some influence. A vigorous and decisive personality and an exceedingly able practitioner, Adler had always had a hankering for a research career, and he saw in Carl Alsberg a sort of spiritual son to fulfill the ambition; he even offered to finance him. Carl's own inclinations ran strongly the same way. He would have made a successful doctor and found happiness in the work, but his bent toward investigation was stronger. He spent nearly three years abroad, mostly with Oswald Schmiedeberg at Strassburg and with Emil Fischer in Berlin. The former—whom Carl later called "the father of modern pharmacology"—wanted to keep him on an appointment, but recognized that there was no academic future for him in Germany.

In 1902, Alsberg returned to this country and began a six-year connection with the Harvard Medical School. For three years he was assistant in physiological chemistry, beginning with a salary of $400. The next three years he was instructor in biological chemistry, first on a one-year appointment and then on a three-year, with an eventual salary of $1,600. These were happy and fruitful years, yet not wholly satisfying.

With the encouragement of President Eliot, then nearing the end of his long incumbency, Alsberg had refused an offer from Columbia. In August 1908, he wrote Eliot of another offer—to take charge of the pharmacological work of the Bureau of Plant Industry in Washington. This field and its opportunities strongly attracted Carl, and he welcomed the prospect of research assistance and freedom from administrative duties. He had, however, "no special desire to leave Harvard." To this phrase Eliot reacted vigorously: "The Corporation does not desire to retain in its service any man who does not distinctly prefer their service to any other which is open to him." Eliot assured him that his position in the School was secure, and that advances in title and salary would be sure if somewhat slow. He offered the prospects of reductions in administrative work and a shift into pharmacology. But he considered that Alsberg should be satisfied with his rate of academic progress, and was unwilling to recommend immediate advancement in rank and salary which Alsberg felt that he had fairly earned. These views being, as Eliot finally wrote, "incapable of being brought to coincidence," Carl resigned and went to Washington.

At Harvard he roomed with Henry V. Hubbard, the landscape architect. L. J. Henderson, Walter B. Cannon, and the Hugo Münsterbergs were among his close associates. So was Felix Frankfurter, with whom the bond grew through the Washington years. Professionally he collaborated with that frail genius, P. A. Levene, one of the original members of the Rockefeller Institute for Medical Research, with whom he had worked during his Medical School days, when Levene was chemist to the Laboratory of the New York State Hospitals. They published together, and discussed the philosophy of science; and Levene remained his mentor to the end.

Within four years, President Taft appointed Alsberg Chief of the Bureau of Chemistry. Harvey Wiley, its first Chief, had given this organization valuable publicity under the Pure Food and Drugs Act, but he was intransigent and had become old. The times called for a chief with flexibility and more fundamental knowledge. There was some little stir and outcry. When Wiley was reported critical of the appointment, Alsberg made a friendly statement that he did not believe Wiley could have been correctly quoted.

What Alsberg did with the Bureau of Chemistry, Linton has competently set forth. It was a task that called for endless tact, sound judgment, immense knowledge, and equal measures of justice and conciliation. First-hand experience gave him training in both economics and politics. From anecdotes which he told in after years, one might infer that his special gift lay in frictionlessly blending the advancement of science and the public good, and that clashing special interests always came to realize that they were being dealt with in complete fairness.

Apart from some transient interests, apparently rather inactive by most standards, Alsberg had always behaved as if women were just people. His marriage during the Washington years to Emma Blount Peebles came as almost a shock to his friends. A Tennessean, raised in an atmosphere where men understood horses, whiskies, and politicians, she was in many respects Carl's total counterpart: precise where he was casual, shy almost to retractability, abrupt, salty, trenchant in her unerring judgment of people, while he remained phlegmatically amiable and all-embracingly discursive in his curiosities. Perhaps partly because of the temperamental contrasts, certainly because of equal loyalty on both sides, the marriage was a happy one. Their rippling little mutual teasings before friends evidenced a deep, steady affection that shrank from direct demonstration. She died a lonely widow in March 1946.

Alsberg resigned from the Bureau of Chemistry in 1921 because he did not want to remain predominantly an administrator. At Stanford University, the Food Research Institute of which he became co-director—and there are few institutions which can boast of sixteen years of harmonious living under three joint directors—offered more time for the laboratory, advanced students, and a research environment. He left there to direct the Giannini Foundation only because the statutes of the University of California allowed him five years more of prospective activity before retirement than those of Stanford.

How far his hopes were realized it is difficult to say. They were balked in part. He did get back to the laboratory, but less than he wanted: economic and social problems were forced on him. He kept investigating and publishing—in his own name, with assistants, and through students. But his counsel was wanted by institutions and governments, by congresses and commissions and planning bodies; and it was impossible to deny them all even if he had wished to do so.

He spoke of himself at times as a failure—as a good scientist turned into a poor economist. So far as formal distinctions like membership in the National Academy of Sciences are concerned, there is little doubt that the partial shift-over deprived him of some. What he surely recognized, however, though he was too modest to voice it, was that the loss to biochemistry and pharmacology was the manifold gain of economics, in being brought into genuine contact with fundamental science through his instrumentality. Alsberg was always ready to serve the wider cause. In his youth, he heeded the call of total science as against that of his professional field; in his maturity, that of public and national interest.

When he was about sixty, we once drifted into a discussion of success, and astonished each other by our mutual confessions that we both had had ambition but had thought the other free of it. I am sure we were both right about him. He was healthily ambitious, but never exploitive and scarcely even competitive.

Others can better appraise Alsberg's career, as it unfolded, and its influences on American life and science. I have tried to set forth some of the antecedents and factors that shaped the career. In addition, there were the traits which expressed his personality and remained constant in boyhood, youth, and maturity.

In first place was his character, of extraordinary probity and solidity. His intellect was at once plastic and massive, aided by an unusual, effortless memory. He was in no sense brilliant or facile; he worked steadily. It was almost impossible to excite him. To some he seemed phlegmatic. He was not to be hurried, hence was often in arrears for time. His friend and classmate Joseph M. Proskauer thus inscribed Carl's copy of the first volume of *The Morningside* in June 1897:

> O kindest of kindhearted mortals,
> A heavenly seat you will win;
> But surely you'll reach heaven's portals
> An hour too late to get in.

Most marked was his judgment, as free as can be encountered both from prejudice as regards persons and from bias as regards ideas. Jealousy, spite, and hate were foreign to his makeup; hence his repute for complete fair play and for wisdom in counsel. He understood men; he appraised them, but never judged them.

His imagination was not revolutionistic. It was directed less to basic innovation than to sound combinations and adjustment, and to steady, fruitful pressing ahead. Equally unusual and unobtrusive was his gift for simple, basic organization. Unobtrusiveness pervaded his personality. His heavy-browed face, almost ascetic in youth, more rounded in age, surmounted a large, strong, long-limbed body. His features broke readily into smile, his laugh boomed deep, but his habitual expression was one of slightly melancholic gravity, lightened by kindly, seeking eyes. A man's man, intent only on going his own way, he forged into leadership through sheer weight of ability.

Work in the Natural Sciences

By

DONALD D. VAN SLYKE*

Carl Alsberg was a man of tremendous versatility of mind and breadth of information. Trained in medicine and pharmacology, as the son of a chemist he imbibed an interest in chemistry which pervaded his life and dominated most of his work. It is therefore natural that he should have accepted an assistantship in physiological chemistry in the Harvard Medical School after his return from Germany, where he had declined the offer of an assistantship in Schmiedeberg's laboratory—an offer such as was seldom received by an American in Germany. In his last three years at Harvard, with only an instructor's rank, he and L. J. Henderson headed the new Department of Biological Chemistry, and Alsberg became one of America's pioneer biochemists in spite of his preference at the time for pharmacology.

The last three summers of his Harvard period he spent at Woods Hole, Massachusetts, investigating the food values of seafoods for the United States Bureau of Fisheries, under Francis B. Sumner. From there he went to the Bureau of Plant Industry in the Department of Agriculture in Washington, first as expert in chemical biology and, after a year, as chemical biologist in charge of the Poisonous Plant Laboratory. He often later remarked that there is not much difference between the fundamental chemistry of plants and that of animals.

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* Research chemist, Rockefeller Institute for Medical Research since 1907, and chief chemist of its hospital since 1914; past president of American Society of Biological Chemists and of Harvey Society of New York; visiting professor at Peking Union Medical College, 1922; president of American Bureau for Medical Aid to China, 1941–47; Willard Gibbs Medal, 1939.

Alsberg was an active participant in many scientific and professional associations. In 1904 he was secretary of the Section on Physiological Chemistry of the International Congress of Arts and Sciences at the Louisiana Purchase Exposition at St. Louis. While at Harvard he served as secretary of the Boston Society of Medical Sciences. His activity in scientific societies increased greatly during his first four years in Washington. By 1911 he was one of the vigorous younger leaders in the American Chemical Society—councilor representing the Washington membership, chairman of the Section of Biological Chemistry, and an associate editor of *Chemical Abstracts.* He was a fellow of the American Association for the Advancement of Science. His range of scientific interests at the time is more fully reflected in his membership in the American Physiological Society, the Society of Biological Chemists, the Society of Pharmacology and Experimental Therapeutics (of whose *Journal* he was a "collaborator"), the American Pharmaceutical Association, the Corporation of the Marine Biological Laboratory at Woods Hole, and in his associate membership in the American Medical Association and the American Association for Cancer Research. In several of these organizations his membership was far more than nominal, and in later years he was active in several of these and others besides.

Alsberg's presidential appointment at the age of thirty-five to head the Bureau of Chemistry in the Department of Agriculture was heavily due to the warm and widespread support of his candidacy by a number of leading scientists who had come to know him, who rated him high as a man, and who prized as well his scientific output and his stimulating leadership in several fields of research. This once in his life he actively sought an appointment. The reasons were thus set forth in his letter of April 15, 1912, to an older professor in the Harvard Medical School who doubted that the shift would be in Alsberg's interest:

> . . . . Two things must be kept in mind. One is that while the authorities may not care whether scientific work is connected with the position or not, the man who gets the position, whoever he may be, will have it in his power to have done and to stimulate a great deal of good work. He will have more power inside his own organization than you seem to realize. The second thing is that the man who is appointed will hold the position many years if he wishes. The present attitude of politicians and the public cannot last. The man is bound to stay long after public opinion has changed. If he is a thorough scientist with good ideals he will then be able to do an immense amount of good. If he is not that type of man the situation will remain hopeless. The appointment should be made with an eye on the future rather than the gloomy present. If I felt the men in line for it were thorough scientists with good ideals I would not bother my head about it for a moment. This is, however, far from being the case. There is no scientist in the field who has a very good chance. My friends tell me that I have a better chance than any of the other scientists. Under the circumstances, since my name was brought forward originally without my knowledge I feel it my public duty to make as strong a bid for the place as I can, even though, should I by any chance be chosen, I would not be particularly comfortable in it. That the chances of a man with low ideals getting the place are so good, is the very reason that makes me wish to make a hard fight for the place. At least that is the way I react to such a situation. Therefore I want to ask you whether there is not any way in which you can help me. Would it be

> too much to ask you to write to Mr. Taft about me? Possibly too you might care to communicate with Senator Crane or Representative McCall. Anything that you might care to do in the matter I would very greatly appreciate.

Alsberg took up his new, challenging task with the firm intention of developing the Bureau of Chemistry as a scientific institution, in full harmony with its special duty of enforcing the Pure Food and Drugs Act of 1906—to which his predecessor had devoted most of his energies. The heavy responsibilities of this position necessarily limited his direct, personal contributions to scientific knowledge. Yet he never ceased to keep his hand in the laboratory; and even during the most strenuous periods of his administration of the Bureau, scientific papers appeared under his name.

Alsberg shared many of the ideas about a scientist's writing that Walter B. Cannon, his contemporary and colleague at Harvard, long practiced and later expounded in his informal autobiographical work, *The Way of an Investigator* (1945). In August 1914, Alsberg wrote to E. H. S. Bailey complimenting him on his recent book, *The Source, Chemistry, and Use of Food Products*:

> . . . . I have gone through the book carefully and am convinced that it will be exceedingly valuable as an educational factor, not merely in such educational institutions as will no doubt adopt it, but for the intelligent citizen as well. It seems to me that you have hit the fortunate medium that lies between a technical exposition and superficiality that usually characterizes such books. You have been particularly fortunate in producing a book which is scientifically accurate and yet which is easily understood.

These words suggest an ideal which Alsberg held before himself as he labored in a variety of fields, and which he urged upon younger men who worked with him or under him.

Two of Alsberg's early papers were prophetic. He reviewed "Recent Work in Biological Chemistry" in the 1910 *Journal of the American Chemical Society* (17),* and he contributed to Appleton's *American Year Book* for 1911 the section on "Biological Chemistry" (100). Twenty-one years later he warmly seconded J. Murray Luck's proposal to establish the *Annual Review of Biochemistry*, persuaded the Chemical Foundation to provide for it a guaranty fund without interest, and actively served on its advisory committee, board of directors, and editorial committee until he died. Its success led Alsberg later to urge (thus far in vain) a similar annual review of agricultural economics. Some years after his death this idea contributed to the launching of a project of the American Economic Association for the publication of a *Review of Economics*, the initial volume of which is expected to appear in 1948.

After his acceptance of a directorship of the Food Research Institute at Stanford his administrative duties were light, but his commitment to scientific spheres beyond the bounds of chemistry was increased. His publications during this later period show his participation in the broadest economic problems. Nevertheless the continuity of his contributions to natural and social science bears witness to the fact that Alsberg remained throughout his life a scientist and productive investigator. He gave the best of his versatile mind to the questions that his duties imposed upon him; but always he managed to reserve part of his vitality for the experimental solution of problems.

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* Reference numbers in this and the following paragraphs, as well as on later pages, indicate titles to be found in the classified bibliography beginning on p. 155.—Ed.

His first paper was on work done while still a medical student as a volunteer collaborator with P. A. Levene on the nucleic acid and the vitellin of egg yolks (1). The interest which this work aroused in Alsberg for the chemistry of the proteins and of organic phosphorus compounds in nature can be traced through the rest of his life. His research during his two years with Schmiedeberg was on the nature of the nucleic acids (3), and he then returned to work with Levene once more on the vitellins (5). This work included one of the first protein hydrolyses done in this country by the then novel ester method of Emil Fischer. At that time there was no accurate knowledge of the rates of hydrolysis of proteins in boiling mineral acids, and Levene and Alsberg published what was probably the first systematic observation of the time course of such hydrolyses (9). The interest aroused in the vitellins of birds' eggs led Alsberg later, with E. D. Clark, to apply his experience to the proteins of fish eggs (10).

A paper published with Folin during nearly the same period ended a controversy concerning the nature of the cystine in the urine of cystinuric subjects. It had been reported that this cystine did not have its amino group in the alpha position and its sulfur in the beta, as in the cystine yielded by protein hydrolysis, but that the sulfur was alpha and the nitrogen in the beta position, and that the product was excreted in the urine because it was different from the "natural" cystine yielded by protein hydrolysis. Alsberg and Folin (4), on the contrary, showed that cystine from urinary stones was identical with the cystine from hydrolyzed proteins, and that when cystine preparations obtained from both sources were ingested they had the same effect on cystine excretion. This paper also showed the lack of any simple relation between cystine intake and cystine excretion in cystinuric subjects, and stimulated an interest in sulfur metabolism, which had much to do with the important part that American biochemistry later developed in that field.

From the vitellins Alsberg, during his summers at Woods Hole, went on to a study of the blood proteins of that old Atlantic Coast relic of a former geological age, the horseshoe crab. With E. D. Clark (11, 18, 38, 42), he showed that the protein picture of this animal's blood is peculiarly simple, consisting of the blue copper-containing hemocyanin which replaces the hemoglobin of more modern animals, and the fibrinlike protein which coagulates when the blood clots. With William Mansfield Clark (39), he also studied the solubility of oxygen in the horseshoe crab's blood, and pointed out that it was held with great tenacity, a characteristic which has later been found by Redfield to be common in marine animals that live at low-oxygen tensions.

In later years Alsberg's interest in the proteins was transferred to those of importance in foods, and the results were evidenced by a series of papers on the properties of the proteins of grains and other foods (73, 76, 81).

With his broad interest in all phases of physiological chemistry, it was inevitable that Alsberg should encounter problems involving enzymes. His first paper in this field was one of the first to indicate a role of enzyme abnormalities in pathology; this publication was on the occurrence of oxidative ferments in a melanotic tumor of the liver (7). He revealed the suggestive fact that the extract of such a liver contained an enzyme which, in the presence of oxygen, darkened pyrocatechol solutions. Another publication in the field of oxidative enzymes was his contribution to the *Festschrift* of his old master, Schmiedeberg. This was a severe, experimentally supported criticism of the use of the guaiac reaction in studies of oxidative enzymes (12), for which guaiac, up to that time, had been a popular reagent. Later, after his interests included agricultural and food chemistry, we find Alsberg engaged with Stone on a study of the action of rennin (85), and with Sahyun (95) on the hydrolysis of glycogen by diastase.

Soon after beginning work at the Bureau of Plant Industry, Alsberg began to publish on the metabolism of molds and bacteria. His first papers contained the discovery that the olive tubercle organism oxidizes glucose, not to CO_2 and water as does the animal organism, but only to the stage of gluconic acid (13, 21). The possible connection between consumption of spoiled maize and nutritional diseases led him, with O. F. Black, to studies of the products of penicillium and to the crystallization of two new phenolic acids, each characteristic of one species of penicillium (24, 28, 32). This work showed how difference in metabolic products can distinguish between two fungi otherwise almost identical. Later studies included an investigation with Krueger (66) of the amines produced by *B. solonacearum* by means of analytical methods which Woodward and Alsberg (54) had devised.

It is obvious that the practical importance of starch in the food problems which became Alsberg's responsibility aroused his interest in the properties of this substance. Besides the practical applications of starch chemistry to economic and nutritional questions, he produced, partly alone and partly with collaborators, a series of papers devoted purely to studies of the physical and chemical nature of the starches (62, 67, 77, 79, 82, 99, 102, 111). In these papers he demonstrated, among other things, that starch pastes are not colloidal solutions, but suspensions of swollen and softened granules, and that breaking up the granules of dry starch by fine grinding produces a product which dissolves without the viscosity that characterizes solutions made by heating the unbroken granules in water.

Allied with his studies of starch were those of glycogen (86, 88, 95), in which he developed improved methods of purification; in particular, the extraction method with trichloroacetic acid (88), which yields a nearly ash-free product. The nature of the hydrolysis of glycogen by diastase was also fruitfully studied (95).

Alsberg's training in pharmacology found repeated application in his successive fields of work. He isolated a toxic alkaloid from *Amianthum muscaetoxicum*, and studied its physiological action in detail, showing that it kills by respiratory paralysis (26). With Black he reopened the question of the toxin of the locoweed, which had previously been accepted to be its barium salts. He showed that the amounts of barium present in the locoweed had no toxic effect, and the field was reopened for investigations on the true nature of the toxin (30). The saponins aroused his interest, and brought about a study showing that the blood of animals fed on saponin develops resistance to saponin hemolysis, the resistance being due to a substance formed in the serum (37). The toxicities of cadmium and zinc, because of their possible roles in canned foods, were thoroughly studied. In experiments with different animals, the toxic and lethal intakes were ascertained, and detailed physiological and pathological effects of acute and chronic intoxication (56, 57). The immense use of cottonseed oil cake for stock feed made the occurrence of toxicity in the seed a matter of prime economic importance. The cause was studied by Alsberg and Schwartze (58, 60). The poisonous effect was shown to be entirely due to the gossypol content, and a thorough pharmacological study was made of the detailed effects on animals.

Alsberg's breadth and versatility are indicated by the readiness with which he progressed from pharmacology and animal physiology to problems in the physiology of plants, when his field of work shifted to agriculture and foods. His problems in plant physiology cover such varied subjects as cyanide production in grasses (40, 45, 46), for which, with Black (45), he developed special analytical methods, the amino acid contents of food seeds (48), the bitter glucoside of olives (96), the effect of environment and heredity on wheat composition (116, 121), and a critical discussion of exponential growth formulae as applied to plants (80).

Characteristic of Alsberg's scientific work was the application of a fresh point of view to one problem after another that he encountered as a matter of observation within or without the laboratory, or that he met in connection with the duties of his positions. He never multiplied publications merely by repeating observations on related materials, or with modified methods. When the question that awakened his interest was answered, as far as means were available for its answer, he strode on to other fields.

Carl Alsberg's ability to inspire his younger colleagues was such a powerful factor in his contributions to science that it is impossible to speak of one without the other. My own first contact with him will never be forgotten. Through his influence my first chief, P. A. Levene, arranged for the summer of 1908 that I should have a similar position as investigator at the Woods Hole Laboratory of the Bureau of Fisheries. The first day that my young wife and I spent there was one of depression. Alsberg had not yet come down from Harvard, and we knew no one. The laboratory assigned was little but bare walls, and there was no one to suggest what could be done. We signed up for board at the "mess," which was gastronomically discouraging. After our first dinner we hung our heels over the wharf and debated whether we should face the summer or take the next boat back to New York. We decided to try another day. The next morning Alsberg arrived and life took on a different hue. Carl, with his mother, his sister Elsa, and his brother Henry, had a cottage, where Mrs. Alsberg presided over a famous cuisine, and many of the leading biologists of America dropped in for evenings of inspiring give-and-take discussion of problems that ranged the world. Carl invited us for luncheon, where a memorable sauerbraten filled the caloric vacuum left by our encounter with the mess. Then with Elsa he took us out in their catboat for a cruise, in a glorious breeze over the whitecaps of the sun-sparkled Sound. There followed an evening that made us forever part of Alsberg's circle. The next day he spent with us, discussing problems to be attacked and planning the construction of a Kjeldahl still and hood with Davis, the man in charge of the physical plant of the laboratory, one of the jack-of-all-trades mechanics who were once a feature of American life.

That summer became one of the most joyous episodes in our memory. And through the years that followed no opportunity was missed to renew the contact, always with a revival of the inspiration that began with that first day. Although Alsberg was only a few years my senior in age, his heritage of combined European and American science and culture, and the immense richness of his wisdom, made an evening with him seem always like a re-education.

Alsberg's files contained a letter from William Mansfield Clark which expresses the depth of Alsberg's influence in more adequate words than I can muster. Clark, with whom we shared the happiness of Alsberg's friendship at Woods Hole, wrote him in later years:

> I wonder if you remember a hot night in Woods Hole when I confessed to you that the Fish Commission had given you a mighty ignorant assistant, and you took me up to your room to tell me about the beginnings of biological chemistry and how Spallanzani discovered pepsin by the mean trick of tying a string to a bait and deluding a duck? When you play with your grandchildren and fame has lost some of its glamor, please recall that night as one on which you started a kid in his ambition to learn of the heritage left us by Spallanzani, Bernard, and Pasteur.

Alsberg replied, with characteristic self-underestimation: "I doubt if you can realize how your note has affected me, who have permitted myself to drift into a career in which anything that I may accomplish must be done by stimulation of other men. I know that I am not capable of expressing myself on the subject. I can only say that I shall treasure your note among my most valued possessions."

Chief of the Bureau of Chemistry

By

FRED B. LINTON*

"I act as if I will live a million years, and as if everything I really want will come my way," remarked Dr. Carl L. Alsberg shortly after his appointment as Chief of the Bureau of Chemistry, United States Department of Agriculture. Intentionally exaggerated, his "as if" statement reflected an optimistic philosophy that stood him in good stead as he tackled the tough task before him. That philosophy also profoundly influenced his methods of work.

On December 16, 1912, he stepped from the cloistered atmosphere of a research laboratory to an office desk that was periodically the center of controversial storms of cyclonic intensity. To that desk came pressing administrative, economic, and scientific problems more controversial and more complex than those that came at that time to any other desk in Washington, except perhaps that of William Howard Taft, then President of the United States. The President himself had selected Alsberg for this post, where questions involving vast competing commercial interests, as well as the health of consumers of food and drugs, pressed daily for decision.

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* Administrative assistant to the Chief of the Bureau of Chemistry throughout Alsberg's tenure of that office and that of his successor, Walter G. Campbell, and to the latter as Commissioner of Food and Drugs until Linton's voluntary retirement on June 30, 1944, after 42 years' service in the federal government.

Two administrations had been shaken, and one of them nearly shattered, by controversies growing out of decisions under the Food and Drugs Act made at that desk by his predecessor. That law, since its enactment in 1906, had been administered, at first actually and later nominally, by Dr. Harvey W. Wiley, a zealous crusader. As a direct result of the bitter controversies that had been aroused, Wiley had been shorn of most of his authority through the appointment of a Board of Food and Drug Inspection to make administrative decisions, and a Board of Consulting Scientific Experts to settle scientific controversies. This division of authority not only bred increased controversies but also retarded the effective enforcement of certain features of the Act, notwithstanding the progress made in eliminating many of the grosser forms of food adulteration. Wiley had resigned in March 1912, and R. E. Doolittle had served as Acting Chief of the Bureau until Alsberg assumed the duties of his position. Not long after, both Boards were abolished, and increased authority was delegated to Alsberg by the Secretary of Agriculture, in whom the Act placed primary administrative responsibility.

Alsberg had been selected by the President for this important and responsible position upon the recommendation of some of the leading scientists of the country, who knew him for his sound scholarship, his broad humanitarian outlook, and his able research achievements. He was practically unknown to the leaders in the various food and drug industries regulated by the Act, and wholly unknown to federal and state food and drug control officials, and to the general public. Although he had been on the staff of the Bureau of Plant Industry of the Department of Agriculture for four years, his researches had not brought him into contact with the work or the workers of the Bureau of Chemistry, and he was personally known by not more than half a dozen members of the staff of that Bureau. His appointment, therefore, came as a surprise to most of those who were eagerly awaiting the designation of Wiley's successor. It had been generally expected by the staff of the Bureau of Chemistry and by other food officials that either R. E. Doolittle or W. D. Bigelow, former subordinates of Wiley, would be selected, although others had been recommended for the post. Wiley, with characteristic vehemence, had publicly criticized the appointment of Alsberg, pointing out that a man with experience in food law enforcement work, such as Bigelow, should have been chosen.

A group of more than two hundred workers in biological science, however, signed and sent to Alsberg a message reading:

> We who, like yourself, are active workers in the field of experimental biological science, congratulate the country and yourself on your appointment as Chief of the Bureau of Chemistry in the Department of Agriculture. We wish to express our confidence in your ability and integrity. We desire for you a successful administration which shall promote the public welfare, shall jealously guard the public health, and shall uphold the dignity of the science which you represent.

The names attached to this congratulatory message included, among others, such outstanding scientists as Herman M. Adler, Walter B. Cannon, Alexis Carrel, George W. Crile, Harvey Cushing, Otto Folin, William J. Gies, Reid Hunt, Graham Lusk, Lafayette B. Mendel, John R. Murlin, Thomas B. Osborne, Henry C. Sherman, Torald Sollmann, Theobald Smith, and Victor C. Vaughan.

Alsberg approached his first staff meeting outwardly calm but with considerable inner trepidation. He found the heads of laboratories and offices already assembled in his office. He noted that not a man was smoking. Wiley was not a smoker and feared laboratory explosions and fires. He had made and enforced an ironclad rule that no member of the staff could smoke during office hours anywhere in the building. No man ever did smoke in the Chief's office, and any smoking elsewhere in the building was surreptitious. The members of the staff sat around Alsberg's desk, nervously awaiting the breaking of the ice, and wondering, also with some trepidation, what their new Chief would say or do at this, their first formal conference. Alsberg leaned back in his chair and with a twinkle in his eye took up a cigar, lit it, and solemnly remarked: "The rule against smoking is not only hereby broken, but from now on is absolutely abolished." Before the laughter that greeted his sally had ceased, cigars, cigarettes, and even pipes came promptly into the open and clouds of smoke rose toward the ceiling from a group of satisfied smokers now perfectly at ease. By that gesture Alsberg convinced his staff that he was a "regular fellow" and, more important, that, from the first, new precedents were to be established.

It was essential that he win the confidence and support not only of his subordinates, but also of the leaders of the vast industries subject to regulation under the Food and Drugs Act. This he aimed to do by improving and extending the work of the Bureau of Chemistry in developing new processes for the preparation, storage, and handling of perishable food products, and in the more profitable utilization of by-products. He desired to convince the industries that through scientific research the Bureau would solve some of their technological problems and aid them in applying scientific principles to the solution of other problems. He believed that food law-enforcement officials had given, up to that time, too little consideration to the economic features involved in the manufacture and distribution of food.

Alsberg seized the first opportunity to make his philosophy known to the food and drug industries. Addressing an association of manufacturers of medicinal products at the Waldorf Astoria in New York City on February 12, 1913, he began by saying:

> I am particularly glad to make this, my first public statement of policy, before you gentlemen because I believe that the Bureau of Chemistry has no more important task than the problem of dealing with drugs and medicinal preparations. The brevity of my tenure of office renders what I am about to present to you, I regret to say, merely an outline of the principles by which the Bureau of Chemistry is to be guided. I would far prefer at this time to make no statement at all but to wait and let the work of the Bureau speak for itself. As it is, I feel like a man who is "kiting" a check. I hope that the checks which, in behalf of the Bureau, I am about to make out to the order of the people may be amply covered before they are presented for payment. I realize fully that this will be no easy task.

Then, speaking for the benefit of the food industries who he knew were also listening in, he said:

> The Bureau hopes to co-operate with them in placing their processes upon a scientific basis, in eliminating waste, and in utilizing by-products to the best advantage. I would say that the Bureau hopes to do for them what other Bureaus of the Department of Agriculture are doing for the farmer. Great benefit to the consumer must result from such a course.

Complimenting the medicinal manufacturers on the investigational work they had already done in their own chemical laboratories, he continued:

> Many of you realize that the time has come for the co-operation of the medical investigator and the manufacturer of remedial agents. It may be that there are some who fear that such co-operation will commercialize the medical profession. I am not one of them. It has been amply shown that the solution of many therapeutic problems may be largely a matter of money. It often takes tremendous resources to carry such work to a successful issue. Few private investigators and few institutions command them. Here manufacturers may and do step in and help the investigator. As you all know, some of the greatest therapeutic advances of recent years, notably Paul Ehrlich's Salvarsan or 606 and the coal-tar products, would have been impossible except for such co-operation. The fact that the manufacturer offers his help as a speculation by which he hopes to gain does not alter the fact that the net result may be of immense benefit to mankind.

He then turned to the crying need for better legal regulation of the manufacture and sale of drugs and their preparations, particularly patent medicines and the narcotics. Contrasting the excellent results already attained in preventing the graver forms of fraud and adulteration in foods through law enforcement with the little progress made in the control of medicinal preparations, he stressed the limited authority of the federal government to regulate drug products and predicted that the recently enacted Sherley Amendment would prove inadequate, pointing out that it did not cover advertising. He went on:

> Gloomy as the outlook seems, there is yet a ray of hope. It is the attitude of a small but powerful portion of the press itself, which has voluntarily scoured its advertising columns

till they contain only clean and honest matter. The movement is spreading. Let us hope that its progress will be so rapid that it will make legislative control of advertising unnecessary.

That public sentiment and a sense of justice and public service induce individuals to sacrifice their own self-interest, is one of the hopeful signs of the times. It gives one a thrill to realize that this spirit is not peculiar to the press which one expects to fashion the country's ideals but that it is to be found everywhere. It has been exemplified in a most striking way by one of the largest concerns of the country, which has announced that it will no longer deal in patent medicines. It has substituted non-secret preparations of recognized merit. Let us hope that others will follow the trail thus blazed. I am proud to say that the chemist of this concern, who, I am told, was probably involved in this pioneer reform, was formerly a scientist in the Bureau of Chemistry.

Alsberg realized that his speech would be of interest and concern not only to the group to whom it was immediately addressed, but also to other industries, to state and city food and drug control officials, to scientists, to consumers generally, and especially to a large group of devoted followers of his predecessor who looked upon Wiley's zealous pronouncements as the only pure, unadulterated doctrine in the field of food and drug law enforcement. For the latter group he said:

The work of the Department has, I think you will all admit, been very successful in improving the quality of our foods and preventing the grave forms of fraud and adulteration. We realize fully that much remains to be done, but public confidence has been won; and precedents have been created. No backward step will be taken. There will be no let-up in the enforcement of the Pure Food and Drugs Act.

He took great pains in the preparation of this address. After he had written it in preliminary form he consulted a number of associates in the Department of Agriculture about its substance. When he came to put it into final form he obtained the assistance of a personal friend who had already won his spurs as a newspaper reporter, as an author of fiction, and as a public relations counsel. He observed that the newspapers, in commenting on the speech when it was released, played up several of the sentences which he had changed in form at the suggestion of this friend. That gave him an idea.

He recommended to the Secretary of Agriculture that the Bureau of Chemistry employ an information specialist, the employment of press agents being prohibited by law. The duties of this specialist would include the task of translating administrative policies, legal regulations and decisions, and especially the findings of scientific specialists into brief, interesting, informative language readily understandable by lay readers and in a form usable by the press. Considerable persuasion was required to convince administrative officials of the advisability of such an innovation. The few newspaper men who had been occasionally employed as such in the Department were usually those seeking soft berths in minor positions, and their chief function had been to keep unfavorable criticism out of the press, rather than to make available to the press the vast amount of legitimate news and useful information continually developed by the activities of governmental agencies.

So potent were Alsberg's arguments that the Secretary of Agriculture approved the appointment of the man selected by Alsberg, another of his newspaper acquaintances. Moreover, as soon as the Secretary observed how competent the man was, he took him from the Bureau of Chemistry and attached him to the Secretary's office. This was the beginning of the Office of Information in the Department of Agriculture. The first head of this office was George Wilson Wharton, a Columbia classmate of Alsberg's, a news reporter and editorial writer of great merit, who had also served successfully as a public relations counsel for the Superintendent of Schools of New York City.

Alsberg took a leading part in convincing the research scientists of the Department of Agriculture that a practical and skilled newspaper man could write about their beloved specialties in popular language without garbling their scientific statements. The more sensitive of them shuddered at the suggestion that their technical reports, so precise and exact of statement, could be translated into popular language without doing violence to the fine distinctions that were of the very essence of their work. Some of them had suffered agonies from previous abortive attempts by less competent reporters who had seized upon the sensational, rather than the original, phases of their work. Alsberg and Wharton did a splendid job of convincing them that the truth could be told in nontechnical language. Ever since, the work of the Department has been popularized and the results of its extensive research activities made known to the general public through the press, and later through the radio, by a staff of competent information specialists.

Alsberg also recognized from the first that closer co-operation between federal and state food and drug law-enforcement officials would result in greatly increased efficiency in law enforcement, with greater benefits to consumers. By bringing about greater uniformity in regulations and administrative decisions, it would become vastly easier and less expensive for manufacturers of products distributed nationally to comply with federal and state laws. This he also stressed in his first public address.

Acting upon the advice of a member of his staff, he called a conference of the state food and drug officials. This conference, held in November 1913 in the auditorium of the National Museum, was conducted in a most democratic fashion, and all factions were given opportunity to state their views. Out of it came two important achievements which have contributed much to effective food and drug law enforcement. Of these achievements Alsberg stated in his 1921 Annual Report:

> In 1913 both officials and manufacturers complained greatly of the lack of uniformity in the exercise of food control by the Federal and State Governments. Lack of uniformity increases the costs of doing business, and the increased cost is usually passed on to the consumer. It arises not merely from differences in the various laws but also from differences in the interpretation of the laws by the officials and in the application by them of different standards to the same product in different jurisdictions. To meet this situation in part, the cooperation of the Association of American Dairy, Food, and Drug Officials and of the Association of Official Agricultural Chemists was sought. A joint committee on definitions and standards has proposed for the guidance of officials definitions and standards covering a wide range of products which have been adopted almost universally. Thus one cause of lack of uniformity has been eliminated to a large extent.
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> Lack of uniform action on the part of officials and legislators is due not infrequently to lack of information. There was need in 1913 for some sort of clearing house for the exchange of regulatory information, not merely that in the files of the Bureau of Chemistry, but also that in the files of the various State and municipal departments and printed in jour-

nals, bulletins, and reports the world over. There was need for the elimination of a vast amount of duplications and wasted effort due to the fact that officials were not in touch with their colleagues in other localities. Therefore the office of State cooperation was established in the Bureau of Chemistry and the food commissioner of the State of Texas called to take charge. This office has been very successful in drawing officials together, in disseminating information, in securing uniformity, and in establishing effective cooperation among the officials of the country. At the same time the bureau has welcomed such officials into its laboratories, which in many instances have served as a training school for them.

At the time of his appointment as Chief of the Bureau of Chemistry Alsberg was virtually without training or experience in any kind of administrative work, although the position required administrative skill of a high order. Innumerable decisions on a great variety of complex questions were daily necessary. In addition to the daily flow of correspondence, seizure recommendations, criminal-prosecution cases, detention of import shipments, and other documents that came daily to his desk for prompt approval or disapproval, there was the settlement of a considerable number of complicated and controversial questions that had been held in abeyance during the eight months since Wiley's resignation. All these were dumped on Alsberg's desk. He did not yet know the personnel of the Bureau well enough to be certain whose judgment he could accept. A less able and resourceful man would have been terrified by the task that confronted him. Boldly, in fact rashly, he undertook to go personally to the bottom of every question that came to him before making any decision. In the spirit of a true scientist he set out to find the correct answer, and in the meantime he made no answer.

Decisions regarding proposed seizure actions that should have been made at once, if made at all, were made long after the food products involved had gone into consumption, or were never made. Replies to letters from the trade about the application of the law to particular products were prepared by subordinates and sent to the Chief's desk for signature. There they sometimes remained for months, and occasionally for a year or more, before they were signed by the Chief. In some instances the letters were revised before being signed. In others they were finally signed without the change of a word. In every instance Alsberg was satisfied that the answer was right before he signed it. What are a few months in a million years?

He soon realized that there is an important difference between research activities and administrative work in the element of time. The confusion and demoralization that resulted throughout the organization from the long delays brought to his attention the necessity for correction. That he learned this difference well from hard knocks in the school of experience is attested by this passage in his address to the graduating class at Reed College in 1938:

> Many of you will ultimately be in administrative positions. I myself have had to handle a number of such jobs. I think I have learned one or two things I want to share with you. One of them is that if you have five decisions to make at any given time, it is better to make them all with reasonable promptness than to stew over them for weeks and months. It is usually better to make the decisions promptly even if some of them are wrong than to take a long time in order to avoid a mistake. Time is the essence of good administration. It is important that the machine be kept going. Delays and inability to get decisions ruin an organization more rapidly

than a few bad decisions which can usually be rectified. You can't have a good *esprit de corps* in an organization that does not operate. Inaction is the one thing people won't stand for. People will long endure bad government, but rebel almost instantly against a government that can't govern. This, I believe, is the psychology behind much that has happened in postwar Europe.

Alsberg eventually corrected the conditions resulting from delayed decisions, by delegating authority to members of the staff to make all but those of the most importance. While Chief of the Bureau of Chemistry, however, he never became systematic in routing administrative work. He drove his immediate associates to distraction by lack of any sense of time and inability to adjust himself to a definite routine or to regular office hours. On his administrative chores he worked exceedingly hard in spurts. On occasion he would carry home from the office a suitcase full of papers which must have kept him occupied long into the night. He would arrive at the office perhaps at eleven or twelve o'clock the next day with the entire content of the suitcase disposed of in some fashion, usually with a large collection of notes attached to the documents on odd scraps of paper that he happened to have handy when working on the material. These notes, written with colored pencils and addressed by initials to individual members of the staff, usually consisted of short queries very pertinent to the subject matter. Often they caused the recipient to give concentrated study to a phase of the subject that had not before occurred to him.

Alsberg was most democratic in his contacts with subordinates and with all others. He had not the slightest attitude of official superiority. He put at ease anyone who entered his office. He was a sparkling conversationalist and inspired others to confide in him. His good nature did not permit him to be abrupt with anyone. He never could bring himself to terminate a conference. Some of his subordinates, acquaintances, and representatives of food and drug industries imposed upon his time outrageously. He endured long discussions with a manner that seemed to say that discussing the subject under consideration was the most important thing he had to do and was also a pleasure. This was highly gratifying to the person who first entered his office, but exceedingly exasperating to others who were kept waiting long after their appointed time. Scheduling appointments for him kept his secretaries in hot water most of the time.

Unsystematic in transacting routine business, Alsberg was most orderly and logical in methods of study and thought. When he turned his mind to an important problem of organization, of policy, or of science, his solutions were original, sound, and unassailable.

Alsberg came early to the conclusion that a reorganization of the Bureau of Chemistry was essential. He wisely made no move in that direction until he had studied the various operations of the Bureau and had come to know the weak and the strong points of the members of his staff. He could size up the capabilities of men engaged in research with infallible judgment. He was less sure in selecting men for administrative responsibility but did not hesitate to correct any mistakes by later changes. He asked the advice of many men, both in and out of the Department of Agriculture, in whose judgment he had confidence. He sought the form of organization that would best provide the highest efficiency in both research and regulatory operations. On the basis of his own observations and of his own selections from a welter of conflicting advice, he evolved a plan of organization that resulted eventually in improved morale and in increased efficiency in both the law-enforcement activities and the research work. The plan evolved has stood the test of time. In broad outline and in general principle, it is still in operation in the Food and Drug Administration, notwithstanding the great changes that have taken place in the scope of its activities.

When Alsberg became its Chief, the professional staff of the Bureau of Chemistry was made up mainly of chemists and food and drug inspectors. At the head of the organization was the Chemist or Chief and an Assistant Chief. The chemists in Washington were organized in subject-matter divisions and laboratories. There was also a bacteriological laboratory and a microanalytical laboratory. The field chemists were organized in branch laboratories located in twenty-one of the leading trade centers throughout the country. They operated independently, under the very general supervision of the Chief and the Assistant Chief of the Bureau. In the laboratories in Washington each chemist did both research and regulatory work. In the branch laboratories there was likewise no clear demarcation between research and routine analytical work.

The inspectors operated individually throughout the United States under the direction of a Chief Inspector in Washington. While each city with a branch laboratory had at least one inspector, and sometimes more, the chemists and the inspectors operated independently and reported to different offices in Washington. They were expected to co-operate; but the extent to which they did so depended largely upon whether the chief of the laboratory and the inspectors in a given city could or could not work together harmoniously. The results were not all that could be desired.

In 1914, Alsberg organized the field staff, both inspectors and chemists, into an Eastern, a Central, and a Western District, with headquarters respectively in Washington (later moved to New York), Chicago, and San Francisco. Each district was placed in charge of a district chief, who was given authority to direct, and was held responsible for, the work of the entire district staff. The district chief reported directly to the chief of the Bureau. The districts were further subdivided into sixteen station territories, each station territory covering, on the average, three states. Each station headquarters, located in a leading trade center, was staffed with chemists, inspectors, and clerks working under the immediate supervision of a station chief, who reported to the district chief. Each station chief eventually became responsible for all law-enforcement activities within the station territory. In this way responsibility for the work in every part of the United States was definitely fixed.

The Washington staff was also reorganized to separate as definitely as practicable the investigational from the regulatory work. Alsberg believed this essential for best results in both lines. Commenting on the reorganization he said: "The force in Washington then became the planning and supervising department of the Bureau, its general staff, while the field force became the line or the executive department. Most of the research was concentrated in Washington." Emphasizing the need for original research in order to enforce the Food and Drugs Act effectively, he pointed out:

> Such regulatory investigations may in general be divided into four categories: The devising of analytical methods for

the detection of sophistication; the determination of the normal range of composition of foods and drugs in order that deviations from the normal due to sophistication may be recognized; investigations to determine whether or not certain substances added to or accidentally contaminating foods are injurious to health; investigations for the improvement, control, or standardization of methods of food and drug manufacture so that manufacturers may learn to avoid inefficient and insanitary methods, very frequent causes of violation of the law.

In reviewing the effects of his reorganization plan in the *Annual Report of the Bureau of Chemistry* for 1921, he wrote (135):

> . . . . In 1913 the research functions and the regulatory functions were intimately mixed, and one was interfering with the other. Experience has shown that the same individual can not at the same time do scientific research and regulatory work and do them both well. The demands of regulatory work are immediate, those of scientific research remote. Usually research is sacrificed. Moreover, rarely is one person capable of performing both types of work equally well. Apparently one demands a different kind of mind than the other. The situation was cured, simply, effectively, and permanently, by dividing the staff into two fairly distinct services, a research and a regulatory branch. Thus only in exceptional cases has the same individual both research and regulatory duties to perform.

Alsberg had selected two assistant chiefs to aid him in the administrative and supervisory work. One assisted primarily in routine administration, while the other gave major attention to questions arising under the Food and Drugs Act. However, no clear line of demarcation was ever drawn between their respective fields of authority. Friction inevitably followed. Alsberg finally accepted the resignations of both and abolished the two positions. He then promoted a member of his staff, Walter G. Campbell, to a new executive position and gave him charge of the law-enforcement operations and the supervision of that portion of the staff engaged primarily in regulatory operations.* Alsberg was then freer to direct the original research work, which from the first had claimed his major interest in the Bureau.

In addition to researches directed toward finding facts to guide regulatory activities and the formulation of law-enforcement policies, the Bureau of Chemistry carried on research in other fields of wide scope, including problems in agricultural chemistry and in biology. The authorizations were ample, but the funds appropriated for the purpose were limited. Yet Alsberg assembled a small staff of brilliant research men who, working under his direction, made discoveries that were being cited twenty years later by officials of the Department of Agriculture in justification of estimates for funds for continued research work along similar lines.

In the field of agricultural chemistry Alsberg extended researches toward the better or more economical utilization of agricultural products. "Since the improvement of the methods of utilizing agricultural products is of vital importance not merely to the farmer but to the whole country," he declared, "it was decided in formulating the research policies of the Bureau of Chemistry to study the industries that utilized agricultural products, as well as to endeavor to create new or enlarge existing outlets for such products." The research work in this field under Alsberg was never adequately financed, but with only a few thousand dollars he accomplished much. Some twenty years later Congress authorized the expenditure of $4,000,000 annually to build, equip, and operate four regional laboratories for carrying out, on an extensive scale, studies and experiments in the utilization of farm products along lines similar to those laid down by Alsberg. A short time before his death, a representative of the Department of Agriculture consulted him about plans and projects to be carried on in these regional laboratories, then nearing completion. He made helpful suggestions and agreed to be available whenever those in charge might desire his advice.

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* Campbell continued in charge of this work, under various designations, until his retirement in 1944.

Among the most productive research projects extended under Alsberg's direction were those aimed to improve technological processes for manufacturing, preserving, storing, and shipping various perishable food products. Of outstanding value was the work on poultry, eggs, and fish. The canning industry also profited from improved processes developed by the technologists of the Bureau of Chemistry.

Since agriculture is a biological science and even the soil teems with life, Alsberg emphasized the role that biochemistry should play in agricultural research. Knowledge of the complex chemical substance involved in life processes was inadequate. Advances in agricultural and other sciences awaited discoveries in this field. Accordingly, "much of the work of the various laboratories was given a biochemical direction and, in addition, a protein investigation laboratory, phytochemical laboratory, microbiological laboratory, and pharmacognosy laboratory were established." This was the field that most of all appealed to the insatiable curiosity of Alsberg. The work was highly productive. Every research project of the Bureau of Chemistry in this field bore the stamp of Alsberg's fertile mind in its inception, its planning, its execution, and the results attained.

As the reports of the Bureau's researches appeared in various journals, in government bulletins, and in public-service patents, that exclusive portion of the scientific world which heretofore had ignored research in government bureaus, and had greeted mention of it with raised eyebrows, awoke to the fact that they must now turn their eyes toward the research activities of the Bureau of Chemistry if they were to keep abreast of the march of scientific progress in all its branches.

Alsberg was highly successful in convincing backward industrial leaders of the contribution that scientific technology could make to their operations. He established an Office of Development, manned by engineers, to aid in translating facts developed in the laboratory into factory practice. Several manufacturers and trade associations in the food field were influenced to establish their own research laboratories.

David F. Houston became Secretary of Agriculture in March 1913, and Alsberg served under him for seven years. On July 1, 1914, he appointed Alsberg on a departmental committee "to prepare a plan for reorganizing, redirecting, and systematizing the work of the Department," which the latest Agricultural Appropriation Act had authorized. Houston came to have great confidence in Alsberg's judgment. He accepted it without question in matters directly relating to the work of the Bureau of Chemistry. He also consulted Alsberg on a great variety of questions not directly related to Alsberg's official duties, especially those involving scientific problems. So frequently did Houston consult Alsberg about problems arising elsewhere in the Department that it became standard practice for the clerks in the Secretary's office to refer to Alsberg important correspondence arising in other bureaus on certain subjects before bringing it to the attention of the Secretary himself.

On his own initiative Alsberg made frequent constructive recommendations to Houston about matters relating not only to the work of the Department of Agriculture but also to the government service as a whole. He was greatly concerned about the lack of coordination in the scientific work carried on in different departments. Scientific bureaus to a considerable extent had mapped out their own fields of investigation. Each drafted the legislation authorizing its work. The language was often indefinite and might be construed later to cover work not originally intended. The bills appropriating funds to various departments for the work were considered by different congressional committees. Each committee had little knowledge of what other committees might authorize. Not infrequently two or more bureaus were authorized to investigate the same problem. If one bureau was given authority to enter a new field of scientific investigation, competing bureaus quickly asked for authority to enter the same field and cover even more territory. Conflicts inevitably resulted. Too much time of the scientists in the government service was taken up in fighting to protect what they considered their legitimate fields of investigation. Alsberg was greatly concerned lest the confusion and conflicts resulting from such conditions would react to the disadvantage of all scientific work done by the government and make it more difficult to get adequate appropriations for even the soundest research projects. Bureaus of the Departments of Agriculture and Commerce were especially involved in many of the conflicts. The Secretary appointed Alsberg on a committee representing these two departments, and he was named chairman of the committee.

Writing to the Secretary on January 30, 1917, Alsberg called attention to an item in a pending appropriation bill authorizing the President to take action toward a proper and scientific co-ordination of the work of the various executive departments, and said:

> Might I suggest that this may furnish a favorable opportunity to bring about some of the things that I have recommended to you if, in your judgment, they are desirable? This language, if passed, might well be made the basis for the establishment of permanent coordinating machinery for the scientific services of the Government. If it does pass, I will take the liberty of making certain concrete recommendations to you.
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> My object in writing at this time is to suggest that if this matter comes up for discussion, it be treated by the Department not solely on the basis of economies that can be effected but also from the point of view that the proper coordination of the scientific departments of the Government would lead to greater efficiency in that work. In my mind, such coordination cannot be obtained through congressional action. It merely makes the Capitol the battle ground on which Bureaus and Departments wage wars of invasion and defense. It can only be satisfactorily obtained, in my opinion, by the establishment of machinery which will prevent the introduction into appropriation bills of items and language designed to invade the fields of other Government organizations; even when such machinery has been designed it will still be necessary to have for the Government as a whole machinery to accomplish work that is similar to that performed within this Department by the Advisory Committee on Projects.
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> A budget system would unify the action of Congress. Conflict of projects can be prevented only by some such scheme

as is now in use in this Department. No budget can be so specific and no language authorizing an appropriation so definite that an ingenious chief of a bureau, assisted by an able solicitor, will not be able to undertake projects which are not properly within the scope of his office.

Others also urged the establishment of a budget system, but it was not until four years later, in 1921, that the federal Bureau of the Budget was organized and a budget system of a sort established.

When the United States entered the World War in April 1917, Alsberg immediately placed the services of the trained staff and all of the facilities of the Bureau of Chemistry at the disposal of the war agencies. The science of chemistry plays a highly important role in modern warfare. This trained staff was ready to perform a great variety of specialized services needed by the war agencies.

At the request of Herbert Hoover, Food Administrator, Alsberg assisted in creating the organization of the Food Administration. He furnished data regarding food supplies and technical advice about manufacturing processes and substitutes for foods. He gave advice about men technically qualified for many of the important positions it was necessary to establish. Individuals and organizations which could be relied upon to furnish information and advice regarding various phases of the food situation were indicated. A number of men from the trained staff of the Bureau of Chemistry were detailed to assist the Food Administration in the solution of technical problems.

One instance of a great variety of services rendered the Food Administration was in bakery inspections. Through the field-inspection service and the State Cooperative Office of the Bureau of Chemistry, Alsberg was able quickly to use for this task not only the inspectors of the Bureau, but those of the states and cities as well. Thousands of bakery inspections were thus made in a very short time, and the Food Administration was able to enforce its regulations regarding the conservation of flour more effectively than it could have done in any other way. It would have taken many months to organize an independent force for this work, and it would have lacked the experience of the trained federal, state, and city food inspectors.

Alsberg and his staff furnished much technical advice to the War Trade Board in reference to the issuing of licenses for the export and import of chemicals, and of other substances in which chemicals were essential ingredients. Not the least important service rendered by the Bureau of Chemistry to war agencies, in the opinion of Alsberg, was that of teaching members of their staffs technical methods for testing various substances and in conducting chemical investigations.

Alsberg assisted in the organization of the Food and Nutrition Division of the Surgeon General's Office, with officers for the most part commissioned in the Sanitary Corps of the Army. Later he placed the facilities of the regional laboratories at the service of this division, for large numbers of analyses both of foods, for which such data were not available, and of edible wastes. Without this timely help the extensive scientific data on food consumption of the soldier in training, collected by survey parties of officers from the S.G.O., would not have been possible. He assisted also in the organization of the Gas Warfare Service, and a number of the boards and committees. He and members of his staff, being in close touch with various university staffs, had available much information regarding the qualifications of a large number of chemists and other scientists, and were therefore particularly well equipped to aid in securing for war agencies men of special training and qualifications. The war agencies were thus helped to secure very quickly highly trained, well-qualified specialists, and to place them where they could render the greatest possible service.

A detailed account of even more important investigations and laboratory work conducted by the staff of the Bureau of Chemistry under Alsberg's direction in 1917 and 1918 to aid war agencies would require the space of an entire volume. To illustrate its variety, a few types deserve mention: the testing of a wide range of supplies, including foods and drugs for the armed forces; studies of the composition of medicinal products with a view to reducing or eliminating the use of alcohol, sugar, and glycerin in their manufacture; extensive laboratory investigations relative to the drying of vegetables and the preparation of dried soups, dried stews, and like products for use in front-line trenches; the manufacture of sugar for the bacteriologists of the Medical Corps; the production of sensitizing dyes used in military photography; the manufacture of types of charcoal suitable for use in gas masks; the conduct of experiments for the Camouflage Section of the Army; and the furnishing of technical advice to the Bureau of Aircraft Production.

The committees on which Alsberg served during the war also indicate the wide scope of his activities: Interdepartmental Ammonia Committee, which acted in an advisory capacity to the Secretary of Agriculture; preliminary committee formed at the request of the Secretaries of the Army and the Navy to organize the Gas Warfare Service; Interdepartmental Fertilizer Committee, which acted in an advisory capacity to the Secretary of Agriculture; Committee on the Conservation of Tin Plate of War Industries Board; Committee on Industrial Medicine and Surgery, Council of National Defense; Committee on Chemicals, which acted in an advisory capacity to the War Trade Board; Committee on Nutrition, which acted in an advisory capacity to the Food Administration; Advisory Board, Division of Medical Industries; Advisory Committee of the Food and Nutrition Division of the Office of the Surgeon General, United States Army; and Committee of Cooperation, Department of Agriculture and Food Administration.

Alsberg likewise served on National Research Council Committees. He was chairman of the Subcommittee on Chemistry of Foods, and a member of the Division of Chemistry and Chemical Technology, the Subcommittee on Noxious Gases, the Agriculture Subcommittee, the Subcommittee on Physiology, and the Military Subcommittee.

His committee work brought Alsberg into conference with many of both the civil and the military leaders of the federal government during the war. His extensive and thorough information on a great variety of subjects, his ability to analyze questions into their fundamental factors, his resourcefulness in finding solutions to pressing problems, and his faculty for clear statement made him one of the most sought after among Washington consultants. Those seeking his advice included cabinet members, military authorities, bureau chiefs, members of Congress, and other scientists in various branches of the government service. Two of the men with whom he was in most frequent conferences were Felix Frankfurter, later Associate Justice of the

Supreme Court, and Herbert Hoover, then Food Administrator, and later Secretary of Commerce and President of the United States.

Alsberg was of German ancestry, had been educated in part in Germany, and had a great admiration for the achievements of German scientists. Like many others he was therefore subject during the war to the suspicious surveillance of both official and self-appointed secret-service agents. One prominent manufacturer, who had been convicted of adulterating food in violation of the Pure Food and Drugs Act, sought revenge by sending a trumped-up telegram involving Alsberg. Had it been genuine, this would have indicted him of conspiring to reveal to German agents certain information that was being held in the strictest confidence. Inspectors of the Bureau of Chemistry traced the telegram to its source. It had been written by the manufacturer himself. As a member of a national committee meeting in Washington, he had accidentally learned about the confidential information and tried to use it to bring Alsberg's public career to an end. When confronted with the evidence of his part in this despicable trick, the manufacturer was forced, in deepest chagrin, to admit his guilt.

If Alsberg had any conflicting emotions in the early days of World War I, there was not the slightest division in his loyalty after the United States entered the war in 1917. He devoted all his talents and energies to aiding in every possible way the prosecution of the war to a successful issue. Very few individuals rendered greater service to the nation during the war.

Following the close of the war, Alsberg was again free to give his undivided attention to the regular work of the Bureau of Chemistry. Even his optimistic philosophy did not protect him against discouragement under the conditions that confronted him. The extraordinary expenditures for war had made it necessary to pare to the bone appropriations for both research and regulatory activities. Salaries for the most responsible administrative and scientific positions in the government service were tragically low when compared with those in industry, and even below those in universities. Alsberg himself received only $5,000 per annum. The maximum the law allowed him to pay any research worker or member of the administrative staff was $4,500 per annum. The average annual salary paid chemists in the Bureau of Chemistry was $2,275. The cost of living had doubled. He found it increasingly difficult to obtain and hold a sufficient number of men qualified to do the highest-grade work, and he could be satisfied with no others.

In Alsberg's annual report for 1920 (134) he pointed out the struggle "to maintain efficiency despite mounting operating costs, rapid turnover of the personnel, and reduction of the force through resignations for which inadequate salaries alone are responsible." He showed that during the previous year he had lost 35 percent of his staff. Among those resigning to accept better-paying positions were chiefs of scientific divisions, heads of administrative units, and leaders of research projects. They had sacrificed their own financial interests during the war to serve the public, but now the war was over. Men who had been receiving from $3,000 to $4,500 per annum from the government were eagerly sought by industrial concerns for positions paying from $10,000 to $15,000 per annum, and in a few instances even more. "The Bureau has been the country's most important training school for food and drug technologists," Alsberg said in one of his reports.

The food, drug, and chemical industries were eager to obtain the men developed in that training school.

Because of the rapid turnover due to inadequate salaries, Alsberg found it necessary, much to his regret, to use research chemists for law-enforcement work. The pressure for quick action in regulatory operations made it expedient to sacrifice temporarily the research work, but he considered that such sacrifice, while immediately necessary, was unwise from the long-time point of view. He said:

> The neglect of research, which has been unavoidable, will inevitably in time impair the effectiveness of the enforcement of the law, since the bureau is dependent upon research for the means to combat new types of sophistication which are constantly appearing as methods and materials change with the evolution of the food and drug industries. Finally, without its research work the bureau can not render to the general public, the farmer, and the food and drug manufacturer that consulting and advisory service concerning the chemistry and technology of foods and drugs, organic chemicals, agricultural chemistry, and a host of kindred subjects which the country has a right to expect.

Alsberg could not contemplate with satisfaction the outlook for a decline in the progress that had been made in every phase of the work of the Bureau of Chemistry under his leadership. He could see little prospect of obtaining sufficient appropriations in the Bureau of Chemistry during the next few years to finance an adequate research program. This situation led him to consider tempting offers that came to him from agencies outside the government service. In 1921 he accepted one, which he believed would give him opportunity to render greater service than he could render thereafter as Chief of the Bureau. He declined positions carrying much larger financial compensation offered him by industrial concerns to accept a directorship in the Food Research Institute at Stanford University.

The conditions which brought about an exodus of so many employees of the government were later corrected in part by enactment of a classification law which, with its amendments, raised the scale of salaries for most positions, and of a retirement act which provided modest retirement compensation.

When Alsberg left Washington he had won the respect and the esteem of all of his associates and subordinates and the affection of those privileged to know him best. He had more than gratified the desire of that group of eminent scientists who, at the time of his appointment, had wished for him a "successful administration which shall promote the public welfare, shall jealously guard the public health, and shall uphold the dignity of the science which you represent." The checks he had "kited" in the form of promises of performance at the start of his work had been made good by abundant deposits in the form of substantial achievements. Perhaps the best brief summary of those achievements is to be found in his own modest words of 1921, which are the more significant because of their very understatement: "It may, therefore, be asserted in all fairness that since 1913 there has been a steady improvement in the Bureau's research work, in its administration of the law, and in its general usefulness to the country as a whole."

University Professor and Administrator

By

ROBERT D. CALKINS*

Carl Alsberg spent twenty-five years of his professional life in university work. His inexhaustible interest in research propelled him into it. This interest sprang from an inquisitive mind that was forever asking: What is it? Why? and What of it? His central ambition was not wealth, fame, power, or influence, but the desire to know. Upon graduating from the College of Physicians and Surgeons he rejected business and a medical career in order to study in Germany with scientists pioneering in pharmacology and in the new field of biological chemistry. This experience directed his restless curiosity into research so thoroughly that in one way or another he devoted the rest of his life to it.

Next to research his chief interest was in helping others to know. For him teaching was complementary to research. He was convinced early, and to the very end, that the best research men were the best teachers. He found satisfaction in the combination of teaching and research, to each of which his devotion was genuine, undisguised, and unflagging.

When Alsberg left Harvard for the Bureau of Plant

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Industry he had no intention of forsaking university work for good. During his early years in Washington he often thought of returning to academic life. His new work was agreeable enough, but it offered no real substitute for the satisfactions of teaching, the association with students, and the congenial intellectual pursuits of the campus. Recollections of Harvard aroused a nostalgia for the academic atmosphere.

The intensity of this desire to teach he frequently had to gauge, for one institution after another approached him about an appointment. Within a year of his arrival in Washington, he was invited by President David Starr Jordan to fill a vacancy in pharmacology in the Stanford Medical School. Satisfied for the time being, Alsberg declined the invitation. In 1911 he was strongly tempted to accept the headship of the Department of Pharmacology in the University of Minnesota, but these negotiations were interrupted when the university fell into unexpected financial difficulties. He took pains to explore a similar opening at the University of Indiana in the middle of 1911, under the urging of Joseph H. Pratt, who was ready to accept the deanship of the Medical School if he could get good men to serve with him in three important fields. There were other approaches, and some offers. They were not lightly turned aside.

The attractions of teaching had to be weighed against the opportunities for research. Alsberg was a realist. He never exaggerated the advantages of a university position for the conduct of scientific research. Few universities in that period could offer assistants, funds, and facilities to compare with those he enjoyed in the Department of Agriculture. This, on several occasions, was decisive. After his appointment to the

Bureau of Chemistry late in 1912, he found his new responsibilities so engrossing that the appeal of a professorship diminished, and occasional inquiries from universities were more readily given a discouraging response.

When, in 1921, Alsberg next seriously considered an academic career, he had spent thirteen years in the government service. It had diverted him irrevocably from the paths he abandoned upon leaving Harvard. As Chief of the Bureau of Chemistry, he had found diminishing opportunities for his own scientific research, but he had acquired challenging new interests. Official responsibilities brought daily reminders of the wilderness of social problems needing exploration. He saw and participated in the intricate institutional and personal processes of government. Problems of public control and public policy arose for him to solve. He had the responsibility of directing nontechnical as well as technical personnel. To his attention came repeated demonstrations of the value of scientific and technical knowledge in the organized conduct of human affairs. The obscure relationships between technology and affairs he perceived as a vast no-man's land lying between the recognized academic disciplines. On a less inquisitive mind these experiences might have left impressions but little more. To Alsberg they were distracting subjects for speculation and serious thought. They caught his interest and profoundly influenced his subsequent activities.

Compared to the expanding horizons of Alsberg's curiosity, a professorship in a single subject now seemed confining. While he still hoped to go back to the laboratory and the classroom, he recognized the difficulty of returning to his earlier career. This he recorded in a characteristically candid and modest letter to a friend. Writing shortly after he left Washington, he said:

> While I would like to get back into the laboratory more . . . . I am reluctantly driven to the conclusion that the time is past for me to make a new start in experimental research. I have had to give up the hope that I can achieve great distinction through experimental research. I have been out of it too long, my old lines of work have been taken up and carried along by others, and at best my work of ten or fifteen years ago, while it was well worth doing, I believe, now that I look back upon it, was not of a caliber to satisfy me at the present time. Too much of modern chemistry has gone by me in the last ten years for me to have much confidence in a fresh start. My educational equipment, adequate ten years ago, is no longer adequate in the important fields of physical, colloid, and organic chemistry. I am handicapped in everything but administrative experience by my dozen years of government service.

If a return to his earlier career were closed, Alsberg had at least acquired new qualifications and invaluable experience for other work. As a result of World War I there developed a position to which his experience was well suited. It was the challenging sort of new adventure that appealed to him. It came, as other appointments and honors typically came to him, because of his own unusual ability and accomplishments.

During 1917–18, the Food Administration encountered serious difficulties in allocating food supplies because of the paucity of reliable information concerning available foodstuffs. Consequently, after the war, Herbert Hoover proposed to James R. Angell, President of Carnegie Corporation of New York, that an institute be established to make continuing studies of the food supply. The original suggestion was expanded at

Angell's request. Alonzo E. Taylor of the University of Pennsylvania outlined the proposal. Ray Lyman Wilbur, President of the Leland Stanford Junior University, also an associate of Hoover in the Food Administration, recommended the project as appropriate to a university. Considering the project desirable, Angell proceeded to resolve the questions of location and of directing personnel.

The undertaking clearly required an unusual combination of talents. The investigation of the food supply in relation to food requirements manifestly demanded a staff competent in nutrition and dietary matters, in food technology, and in the economics of food production, distribution, and consumption. Taylor, a specialist in physiological chemistry who had had extensive experience in food control and famine relief with Hoover during and after the war, was chosen as the first of three directors. Alsberg's competence in food technology was well recognized, as were his other qualifications. Taylor, who had known Alsberg and his work for a number of years, recommended him for the second directorship. The proposal was promptly approved by Hoover and by Wilbur, who had known Alsberg since the unsuccessful attempt in 1909 to interest Alsberg in a position at the Stanford Medical School. Following this decision, President Angell chose Stanford as the location for the new institute. The search for an economist as the third director terminated in the selection of Joseph S. Davis, then of Harvard University. Financial arrangements were completed and the Food Research Institute was launched in the summer of 1921.

The three directors worked together closely in the joint direction of the Institute. Alsberg's easy-going manner, wide acquaintanceship, quick grasp of ideas, roving interests, deep wisdom, broad knowledge, understanding, and tact were important in the shaping and reshaping of policies and programs of the Institute. His qualifications blended well with Taylor's enthusiasm, energy, broad experience, keen mind, extensive contacts, and great fund of information, and with Davis' sense of workmanship and his perseverance, sound judgment, devotion to practicalities, and executive ability. Decisions were made in close and friendly collaboration, and the resulting policies bore the marks of all three directors.

The field of the Institute—research in the production, distribution, and consumption of food—was so broad that its work might well have taken any one of several directions. The first task was to formulate a manageable and fruitful program. At their first meeting, in the spring of 1921, the directors decided to stress *economic* problems of food production, distribution, and consumption. Rather than investigate a variety of foodstuffs, they decided first to concentrate on a few, and initially on wheat, the leading item of the Occidental diet. All of the directors were convinced that eventually more would be learned about foods if intensive investigations were made of one important commodity, and if the results and experience so acquired were then utilized in the study of others, than if the efforts were dispersed in proceeding, necessarily less intensively, on many fronts at once.

Closely related to this view, that the early investigations should be limited to a few commodities, was a firm belief in piecemeal research. Breaking large problems into manageable projects for study one at a time became a characteristic approach. This method, typical in the natural sciences, was followed in the Institute. It was invariably urged by Alsberg upon graduate students. He was appalled by the failure of young scholars, especially in the social sciences, to analyze data or to delimit their tasks. Addressing a group of economists, he spoke unapologetically of the "ungrateful task" the Institute had elected in preference to broad and extensive studies that invite "brilliant but premature speculations" (181). The thoroughness with which the research of the Institute was pursued is barely suggested by the twenty-seven years of work which have been devoted to the study of wheat and by the 180 monographs published on that subject in the twenty volumes of its *Wheat Studies.*

As the investigation of wheat progressed, Alsberg urged the investigation of fats and oils. With Taylor he wrote the introductory volume in the series on that subject (106). He gave much counsel and assistance on several others, and was joint author with Georges M. Weber of *The American Vegetable-Shortening Industry* (117). He organized and directed studies on rice and on the economic problems of the canning industry. From his fertile mind came an endless flow of suggestions for worth-while research. At times a steady hand on the helm was needed to hold the course and meet the schedule, and for this Davis was relied upon.

While Alsberg hoped to return to the laboratory, his desire was only partially and temporarily satisfied. With one or more assistants and graduate students he conducted or directed numerous laboratory experiments. He made significant contributions to the scientific knowledge of starches, and particularly of the structure of the starch granule. He investigated wheat proteins, the growth of the wheat plant, and the role of fats in shortening. In ten years he published, usually under joint authorship with his assistants, over three dozen notes and technical papers. But his research efforts shifted more and more away from the laboratory. After 1931, when the budget of the Food Research Institute was shrunken, the evolution of his own interests changed the direction of his effort, and the output from his experimental research diminished.

Some of his scientific writing he never completed. The project for a semipopular work on food grades and standards, which he had brought from Washington and had hoped to complete, never came to fruition. His smaller but significant treatise on cassava, which he said repeatedly needed only six weeks of intensive work to be made ready for the printer, failed to receive the necessary polishing. On many lesser projects his voluminous notes remained unorganized.

That Alsberg was not more prolific in research output is to be explained partly by his choice of problems, and in some measure by his restless curiosity, the attraction of new ideas, and his willingness to be enticed from one problem to another before any final conclusion could be reached. Looking back later he could say, in his unassuming way:

> I did not stick to any one line of research long enough to become permanently identified with it . . . . My consuming curiosity has led me to push trails into strange fields of science without ever beating a single one into a broad highway or even a tote road. I've never resisted the temptation to start another trail into another unfamiliar field. And so I haven't become a famous scientist. I suppose I didn't have a sufficiently consuming desire to be famous. But I've had more fun. It is much more interesting to try your hand at many things, even if you don't excel in any of them, than to put on blinders and compel yourself to fix your gaze on only a few degrees of the horizon.

But some of those who knew Alsberg well found in this sincere appraisal an undertone of penitence for not having followed a traditional path to distinction as a great research scientist, an honor well within his capabilities.

Alsberg's view was broad. He saw new fields and wished to explore them. But his shifting interests were in part the consequence of circumstance. He was reluctant to refuse requests to serve on committees, to address meetings, or to go on some mission; and each set him off into a new by-way which led to unforeseen ends. Nevertheless, there was an underlying pattern and direction to his activities.

Less than a year after his arrival at Stanford he declined to become a candidate for the deanship of an important technical school in a leading university. He was contented at Stanford and considered himself more useful there. But he had some misgivings: "The immensity of the field of work of our Institute overwhelms me. Much of it must be in economics and statistics; so little in experimental science. I wonder whether at my age I can turn myself into even an amateur economist and statistician." While "this land of the lotus eaters" frightened him and made him wonder "whether life is not too easy and pleasant here," he was confident that the Institute afforded an opportunity "to capitalize on my government experience" and a "gambler's chance" to do something really worth while. The transfer of his interests to economics was to a considerable extent deliberate, and certainly it was anticipated.

It took Alsberg some time to adjust himself to these new fields of study. At first he "felt like a fish out of water" in the overwhelmingly economic emphasis which the work of the Institute required, and he regarded his own responsibilities as in certain respects "vague and indefinite and unsatisfactory." With the ready approval of the other directors he decided to investigate the subject of crop estimating. This eventually resulted in a study on forecasting wheat yields from the weather (109). His previous studies, including those on pellagra, provided a natural basis for exploring questions of nutrition and the standard of living, and of population in relation to the food supply. One of several papers in this field, written in 1923 and published a year later (163), is reprinted on pages 122–35. Though he published no lengthy discussions of these subjects, they were near the center of his interest for the rest of his life. The Fats and Oils Studies, the first number of which appeared early in 1928 (106), afforded further opportunities for bridging the gap between the natural and the social sciences.

In 1926 he published what he occasionally referred to as his first venture in economics (104). At that time, bakeries of the country were being merged into great combinations with little regard for the economies to be expected from such mergers. Alsberg studied the matter and published *Combination in the American Bread-Baking Industry*, showing the limitations on the size of bakeries and bakery enterprises. The report played some part in terminating the combination movement. It stands today as one of the few highly competent investigations ever made of the economies of scale. Edging farther into the field of economics he wrote on Japan as a producer and importer of wheat (112, 119). His 1931 article on "Economic Aspects of Adulteration and Imitation" (113) clarified this complicated question with an abundance of illustrations from his wide experience.

For the Social Science Research Council, of which he was a member at large in 1934–37, and for the International Research Committee of the Institute of Pacific Relations, of which he was a member and chairman, he wrote and spoke, with increasing frequency, on research problems in the social sciences and on international problems. He became a member of the Commission of Inquiry into National Policy in International Economic Relations (1934) and participated in hearings held in the principal cities of the country. To him fell the task of writing an important section of the final report of the Commission (138). His bibliography now gives only a clue to the extent to which his interests were expanding into the field of economics and the other social sciences. Yet, in spite of these new interests and the interruptions occasioned by them, he found time to complete for publication a number of technical studies and to advance his work on others.

Alsberg always had several research investigations in progress. He alone was aware of the subjects currently commanding his attention. Even his secretary, who was depended upon implicitly to keep his responsibilities in order, knew little of the incidental bypaths into which his curiosity led him. He read and worked at home all hours of the night and often came to his office only a little before noon. He would labor inconspicuously on projects of his own selection. Then, at a staff conference called to consider the publication schedule, he would ask when a *Wheat Study* was needed and offer unexpectedly a choice of several topics on which he had been working unknown to the others. Putting a manuscript into final shape for publication was for him always a chore. He came to lean on Davis for help in this essential function. A passion to have his work all true and strictly true, with no confusion of speculation with fact, made him reluctant to publish until a study had attained such standards. He was overmodest in evaluating his own work. At times he would express doubt that a completed manuscript was worth publishing, whereupon Davis would urge him not to get "cold feet" and would agree to be his "foot warmer."

At his death his files contained folder after folder of notes and partially completed manuscripts on widely varied topics. One excellent but unfinished manuscript on *Standards of Living* remained in his peculiar longhand. Whether he considered it unready for typing, unworthy of publication, or something he should not impose on a busy secretarial staff is unknown. Any of these explanations would have been typical of him.

Taken as a whole, Alsberg's research at the Institute was significant, in some instances excellent; in volume and substance, however, it was not the full contribution of which, with less distraction, he was capable. But if he himself published less, it was because he assisted others more. One of his chief contributions at Stanford was to establish the importance and philosophy of investigation. He inspired men to investigate; he directed research; he coaxed it along; and above all things, as Taylor said, he corrected the work of others.

Alsberg was singularly conscious of departmental lines without being handicapped by them. From his experience in a bureaucracy he knew men, and he knew his way about. For the best approach to a person or subject he had unerring judgment. Through a wide acquaintanceship at Stanford he handled many interrelations between departments. He was an intermediary, marshaling expert advice and co-operation from whatever department they were needed. When he sought information or assistance, he was scrupulously careful to drop in on his informant and never to request anyone, even his juniors, to come to him. Alsberg never tried to make an impression. His simplicity was disarming. The delightful informality of the man, the total absence of selfishness, the complete reasonableness, and the understanding of the other person's point of view instilled confidence. The sincerity with which he could place the welfare of an institution, of science, or of any worth-while endeavor, above minor considerations inspired co-operation. "No scheme of co-operation can work better than the co-operators are willing to permit," he observed. The task of creating the will often fell to him. Yet in these dealings with others there was never a trace of personal ambition, or insincerity, or a desire to wield power, but only an eminent reasonableness in behalf of ends to which he had a fair-minded attachment.

Alsberg's generosity toward his students and assistants won from them enduring loyalty and devotion and inspired them to strive for the highest standards of workmanship. Never too busy to offer advice and guidance when requested, he was certain to suggest helpful leads. "The way to get things done is to plant ideas," he would say. "If an idea is taken up and developed it matters little who gets the credit; the important thing is that the idea be developed." His patience, assistance, and encouragement salvaged no small number of research projects and floundering investigators. He gave credit liberally to those who collaborated with him. In explanation, he said: "I don't need the reputation. They do." And again when asked why he had listed himself as junior author, he replied: "Anyone who knows me will be able to judge my share of the contribution. They wouldn't be able to judge an unknown author's share."

Planting ideas and giving others credit and encouragement were arts in which Alsberg was a master. He enjoyed telling how he had learned these lessons in Washington. "There," he explained, "a wise bureau chief knows that the way to accomplish something is to furnish the idea to those who want the credit for getting things done. In that way both get what they want." Alsberg may have mastered these arts in the government service, but there is ample evidence that he practiced them at Harvard before going to Washington.

Graduate students and younger associates were among those whom Alsberg influenced most. He had an extraordinary capacity to throw opportunities before these young investigators in such a way as to develop their potential abilities and to send them off into new paths of exploration.

A case in point is that of Harold Hotelling. Upon the completion of his graduate training at Princeton, he was appointed to the Institute, at Alsberg's request, because it seemed desirable to have a mathematician on the staff. When Hotelling arrived and asked what mathematical problems the directors wished him to investigate, he was dismayed to learn that they had nothing specific in mind. Alsberg spoke of their interest in crop-estimating and in forecasting wheat yields from weather conditions, but did not advise concentration on these subjects. He advised Hotelling to explore collateral fields for mathematical problems, to keep in touch with the work of the Institute, and to follow out his own ideas. It was made clear that Hotelling was essentially free to find his niche. Later Alsberg brought to Hotelling's attention the work of R. A. Fisher, then largely unknown in this country, and he introduced Hotelling to the work of Lawrence Baas-Becking, whose mathematical studies of biological problems had caught Alsberg's interest. By showing how the findings in one discipline might be useful in another, Alsberg was a fertile source of inspiration. Hotelling, following Alsberg's suggestion, soon found himself deep in the field of statistical theory and mathematical economics, to which fields he has devoted his subsequent career.

How many persons were in this manner lured into new fields of endeavor by Alsberg's speculations and suggestions it is impossible to know. The instance of Hotelling is by no means unique. A host of others had similar experiences. Among them are D. Bruce Dill in the field of industrial physiology, now at Harvard; E. F. Penrose, now at Johns Hopkins, whose work on population was inspired and influenced by Alsberg; and V. D. Wickizer, again at the Food Research Institute, whose research in commodity studies grew out of his association with Alsberg. Later, at Berkeley, Alsberg followed the same technique. There he sought to enrich the subject of agricultural economics by bringing in able scholars from sociology, psychology, and other fields to work on overlapping problems. In this way he hoped to interrelate the fields of science.

Alsberg never forgot a capable student or associate. He went out of his way to find appropriate positions for them. He followed their progress, and from time to time he recommended them for positions which might offer valuable experience or opportunity for advancement. He extended the same consideration to those of his wide acquaintance and to those, personally unknown, whose work had impressed him. Opportunities to recommend people came frequently, for he was consulted about appointments to numerous positions in academic circles, in government, and in business. He was continually being asked to suggest members for scientific and investigating committees, for private and official commissions, and for special missions of one sort or another. He became a broker in men as well as in ideas. As an associate expressed it, he was a forwarding agent for men changing jobs, for ideas in need of men, and for men in need of ideas.

In appraising the abilities of men, Alsberg was exceptional. He was quick to identify originality and knowledge and to see through pretense and superficiality. He disliked flippancy. He occasionally misjudged the ability of a man to do a job and finish it, but he seldom mistook cleverness for competence. While he prized agreeable personality, he felt that in university work, and especially in university research, personality was too often overemphasized at the expense of competence and originality. He often quoted President Gilman of Johns Hopkins, who is reported to have said that he built his great faculty by picking men of originality, paying little attention to personality. Alsberg recognized that brilliant and imaginative investigators are sometimes "queer ducks." But he regarded such scientists and scholars as having singular importance as potential contributors to the stock of human knowledge. A society needing their contributions could well afford to provide opportunities and facilities for such men. He once remarked that if he were running a university and were completely free to choose his own staff, he would surely assemble a queer collection of odd people; but he added: "They would contribute infinitely more to human knowledge than we now typically expect or receive from university faculties."

Alsberg acquired views on the selection of research personnel early in his Washington experience. "Never assign a man to do a research job unless he has a twinkle in his eye, and wants to do it more than anything else," he would say. "If you do, you won't get a good job done." This was the ideal he sought to follow. When he deviated from it, the reason was not infrequently to "do the best you can with what is available." Once an assignment was made and fully explained, he preferred to "leave the investigator alone and let him work." If assistance were sought, he gave it. If the investigator proved unequal to the task, he believed in transferring the man, without loss of dignity, to another position for which he was better equipped. When that was not possible, collaboration often provided a way out; in other cases the project could be cut to manageable proportions in order to salvage as much as feasible. He found it painful to discharge a man and would delay in the hope of finding a happier solution.

These principles reflected an underlying conviction that "you cannot buy research." It is a gamble, and it is safer to gamble on men than on projects. This view he urged relentlessly upon the philanthropic foundations financing and encouraging research. That research was a gamble he could attest by endless illustrations of how purposeful investigations and experiments had turned up highly unexpected results, and how some of the greatest discoveries came about by chance—chance in association with observing, imaginative men. A competent man with an original idea, he argued, could be encouraged and assisted, but a project and funds would not evoke originality. The only hope was in finding the right man, and then being none too sure that the results would be those sought. Nevertheless, he was ready to admit that an organized research program could, when properly staffed, make excellent if irregular progress on an entire front. Of this the Food Research Institute was a demonstration.

Because of his many other activities, Alsberg did not need the stimulus of teaching at Stanford. Nevertheless he enjoyed the association with students, and he annually taught classes during one and often two or more terms. At the outset he gave an advanced course in the chemistry of nutrition. A few years later he offered instead a course on the chemistry of protoplasm. A seminar or special course in colloid chemistry was added from time to time. The chemistry of food manufacture replaced the earlier subjects during his last five years at Stanford.

The extent to which he could roam without going really afield is illustrated by an incident during this latter period. He was invited by the Stanford Graduate School of Business to lecture before a class on Business Policy on the importance of spices to the human race. His lecture was so informative that the following year, and for five years thereafter, he collaborated in offering the course, his responsibilities being to lecture on problems of the food industries.

His courses were not well organized or designed to interest any but the serious student. He assumed that students attended class to learn. He made no attempt to offer intellectual entertainment. His classes afforded students an opportunity to learn of his research, to observe his methods of scientific analysis, and to acquire illustrative data from his extraordinary knowledge and experience. While his classes were never large, those who sat in his lectures will long remember his informative but rambling discussions, his challenging analogies and philosophical comments, and this genial man who so often dropped memorable gems of wisdom.

Alsberg's methods of instruction were well exemplified in his relations with his nephew, an imaginative, restless boy who was looking for an interest around which to build his college studies. The discussions they had at Stanford and the correspondence between them were education at its best. Several of the letters were classic. They exhibited a characteristic thoughtfulness, patience, and genuine interest. In them Alsberg analyzed his correspondent's views, gave clear reasons for disagreement, and offered provocative alternatives for consideration. Appropriately punctuated with the challenge, "What do you think?" they enticed the young man to think through to his own solution.

Alsberg's most influential teaching was informal and scarcely recognized as instruction. At small dinners for students and younger colleagues he would turn the conversation into stimulating and informative discussion. Almost any small meeting that offered an opportunity for the serious discussion of challenging intellectual problems became, in his presence, a sort of seminar. Talking was to him a method of thinking and a way of encouraging others to think. This result he achieved most skillfully, perhaps, in personal interviews, for it was as a counselor that he taught most effectively and exercised much of his great influence over others. He gave hours of his time to visitors who came to him for advice. The interviews with these visitors aroused his fertile imagination and so absorbed his attention that he often forgot the passage of time. Only a considerate guest or a tactful secretary could terminate these discussions; he rarely did.

Undergraduate and graduate students came to

Alsberg for advice on all sorts of questions. Sometimes they came of their own volition. More often they came on the advice of some professor who thought Alsberg might help them, at least to the extent of a worthwhile lead or a reference to be consulted. Young instructors and older faculty members, regardless of rank, came to him in the same manner. He was in no sense merely a director of a research institute and a professor of chemistry. He was in fact, if not in name, a professor-at-large, a *university professor*. If his interest in students had been less keen, he could have been properly described as a professor's professor. Among those who came to him for counsel are not a few mature scholars who gratefully acknowledge their indebtedness, and there are some who claim that Alsberg influenced them and their work more than any other single person.

Alsberg's counsel on scientific and related matters extended beyond academic circles. One of his most useful activities was putting people in search of information in touch with those who had it. To his open door came countless scientists, scholars, public officials, businessmen, writers, reporters, and others, both American and foreign. Arriving, known or unknown, with little or no forewarning, with or without letters of introduction, they would explain their mission, request his help, or ask whom they should see in their search for information. Then Alsberg would lean back in his chair and talk from a masterful knowledge of this possibility, of that difficulty, of an apt analogy, of what might result from one or another approach, and of the over-all significance of what was being undertaken. Or from what seemed like an inexhaustible memory Alsberg would name, one after another, the people who knew something of the subject of inquiry, their family background, education, experience, and connections, the character of their knowledge and how they came by it. He would interrupt himself to ask questions. In the course of the interview Alsberg would find out as much about his guest as the guest would learn about others from him. His personal acquaintances were scattered all over the world. They included obscure students, business and professional men, scientists, statesmen, journalists, and scholars—an altogether impressive list. To them he sent numerous investigators, whose search was aided by his customary comment: "There is a man in Bangkok (or Kiel, or Fresno, or——); I'll give you a letter of introduction to him."

Alsberg's wide academic interests and high standards led to his early election at Stanford to the Graduate Study Committee. This connection drew him intimately into academic affairs. He took a deep interest in the standards of graduate work and he set about doing what he could to attain uniformity in them and to improve the general level of graduate instruction. Being an outsider to all departments, he could, with his customary tact, win co-operation when insiders could not.

He spoke often of how to raise academic standards and relatively little of the need for doing so. Stanford, being dedicated to the ideals of all great universities, was concerned with "the advancement of knowledge and the promotion of scholarship and of scientific research." Alsberg held that there was no inconsistency between training men and advancing knowledge. "Training men to think straight and to develop leaders capable of independent judgment could be done by no better method than training in research," he thought, for research is itself so "dependent on straight thinking, independence of judgment, and close reasoning." And if these were the qualities sought, who was better fitted to give the training than those who practice these skills in their own research? To Alsberg education was a process of developing intellectual competence, not merely of transmitting information. The professor, he insisted, should not be merely a pipeline from the springs of knowledge to the student. "He should show the student where the spring is," so that the student might go to it himself. "It is not necessary for the student to break new trails into uncharted regions," he held. "It is only necessary that he break a trail in a country new to him." Universities, Alsberg believed, should put less emphasis on lectures and examinations and put more on reports, theses, "little bits of laboratory experimentation," and "the solution of problems." The professors should be master workmen who were themselves "real masters engaged in productive research."

Such a university he urged at Stanford. But it was not to be attained by a ukase of the trustees, he explained. It could be attained only by the faculty. He had ideas as to how it might be attained: The faculty must engage in productive research. It should choose younger men capable of creative work—men who do research, not as a means for advancement chiefly, but because they would rather do research than anything else. Instructors who fail to fulfill their early promise should be weeded out, and the faculty should not be deterred in weeding out unproductive investigators because they are "fine fellows personally, or good uninspired teachers, or good administrators." The faculty should not allow itself to become burdened with administrative work or to be distracted from creative work and personal contact with students. The faculty should lead and encourage the young instructor without dominating him. It should reward him and not hold him back because he is a so-called poor teacher or poor administrator, for the poor teacher, not infrequently, is addressing his teaching to the better student rather than the mob, and the poor administrator may be one who prefers creative work to administration. And he added: "We must talk more shop among ourselves, know more about the creative work of our colleagues, stimulate one another intellectually more than I fear we now do." Thus did he lay out his ideas of a university.

In the address to the Advisory Board of the Academic Council at Stanford in which these views were set forth, Alsberg said frankly: "Failure to do creative work because of administrative duties is merely an alibi. It means that the man has elected to do the easy thing rather than the hard one." He failed to note what he fully realized, that there are many roads leading into administration and few leading out. Ironically, only a year later, he was induced by President Wilbur to assume administrative responsibilities as Dean of Graduate Study and Chairman of the Graduate Study Committee. He also became a member of the Advisory Board of the Academic Council. He took over these administrative duties in the hope that he might make his contribution and then withdraw. True to his intention, he resigned the Deanship after six years, but his influence remained, his counsel continued to be sought, and he was never again wholly free from administrative duties.

As Dean of Graduate Study, Alsberg succeeded in improving the procedures and standards for advanced degrees. Several unsatisfactory departmental situations were corrected, and he introduced a procedure whereby doctoral dissertations were to be read and approved by committees of qualified faculty members including professors outside the department or school immediately concerned. He fostered a closer association between the faculty and the graduate students throughout the university. But his primary contribution was in the high sense of scholarship he promoted and in the encouragement he gave to the faculty to improve the quality of instruction. He left a considerable deposit of basic educational philosophy. Today when questions of graduate instruction are discussed at Stanford, the fundamental observations of Alsberg are still quoted. And one may still hear the suggestion, which he occasionally made in a lighter vein, that the university should collect tuition for four years from each new student on the first day of registration and, at the same time, award to each a Bachelor's degree with the understanding that those who desired only the diploma might return home and not clutter up the university, while those who desired knowledge might stay on for four years of study.

Alsberg's resignation from Stanford and his transfer to the University of California at Berkeley, in October 1937, came as a surprise to most of his Stanford friends. He had returned to the Coast late in September after spending three months in Europe. The announcement of his appointment as Professor of Agricultural Economics and Director of the Giannini Foundation of Agricultural Economics came shortly after his return. It was as gratifying to his friends on the Berkeley campus as it was disturbing to his associates at Stanford.

The appointment was the culmination of a careful search by Dean C. B. Hutchison of the College of Agriculture at the University of California, who for some time had been looking for a man of special and outstanding qualifications for the directorship of the Foundation. Convinced of the need for greater emphasis on fundamental research in the Foundation's work, he desired to find a man of imagination and scientific standing who could formulate and direct an appropriate program. As he searched the country for the right man, Alsberg's name was suggested again and again. Alsberg had also been recommended by members of the faculty in Berkeley. An inquiry was put to Alsberg in May 1937, to determine whether or not he would be interested. Rather unexpectedly he signified not only an interest, but real enthusiasm. Before leaving California for Europe, he discussed the matter with Dean Hutchison and a tentative understanding was reached. Upon his return to New York from Europe in early September, he received a definite invitation; and his appointment was confirmed by the Regents, October 15, 1937.

Undertaking new and large responsibilities at the age of sixty, in a field that is largely on the periphery of one's former work, would seem forbidding to most men. To Alsberg it was a challenge. He had an uncanny faculty for visualizing a situation. The problems he would face and the general methods of meeting them he foresaw remarkably well. He relished the idea of undertaking one more intriguing and important job before closing his career. The assurance that he might be relieved of administrative duties at sixty-five and be allowed a professorial career until he reached the age of seventy was distinctly attractive, compared to the mandatory retirement at Stanford at age sixty-five. The stimulus of a new environment, new problems, and new associates he valued highly. Through service on various regional committees he had renewed old friendships and come to enjoy new ones among the large Berkeley faculty. These occasional associations he found stimulating especially on matters currently commanding his interest. He thought well of a closer association. These prospects, collectively, were sufficient to uproot him from his Stanford friends, a comfortable position, and a home—all sources of keen enjoyment.

In the opinion of his Stanford colleagues there was no need for him to have been concerned over the possibility of a responsible position after his retirement in five years. He was alert and vigorous, and seemed to be in good health, as he had been for years. His wide contacts and recognized ability offered reasonable assurance that upon formal retirement he would find attractive opportunities in industry, government, or elsewhere. But he dreaded the thought of inactivity. He preferred to close his active career in a sprint. He spoke of the difficulty of starting anything new at sixty-five. He doubted if he could accomplish much in a new undertaking during the final years after retirement. He was reluctant to be caught in a position of limited choice when retirement came. While his standard of living and his financial needs were modest, he had never experienced financial worries and he preferred to avoid them. Later in Berkeley he sometimes felt that he had taken on a discouragingly difficult job, but he gave no sign of ever having regretted his decision.

Only three years elapsed between Alsberg's arrival in Berkeley and his untimely death on October 31, 1940. Three years were insufficient to permit a notable accomplishment. Since he had taken the Giannini Foundation directorship with the understanding that special emphasis was to be placed on fundamental research in agricultural economics, Alsberg set about the task. Staff conferences were inaugurated for the purpose of discussing the research program. He established a liaison with other departments, in order to interest them in the research problems of the Foundation. He brought the staff into touch with problems beyond their research specialties. Departmental isolation diminished under his influence. A great faith in youth led him to search relentlessly for promising young men who might be developed for the program he hoped to institute.

The plans which were only beginning to take shape at his death contemplated a clarification of the research and the service functions as between the Foundation and the Agricultural Extension Service. He was greatly impressed with the importance of commodity economics, and he hoped to see a strong program developed in that field. To him the excellent start that had been made in studying fruits and vegetables afforded an opportunity to establish the Foundation as a world authority on such products. The study of fruits and vegetables, he believed, would provide distinct benefits to the state; it would give geographical breadth to the research interests of the staff because of the world-wide markets for these crops; and it would create an excellent opportunity to combine the chemical and biological sciences with economics and the other social sciences. But Alsberg was equally interested in land utilization and rural sociology as possible fields for research. He spent considerable time exploring the research possibilities of these subjects. He also took an active interest in the conservation of resources, and participated regularly in a faculty discussion group recruited from various departments to explore these problems. Meanwhile he was on the alert for other appropriate lines of investigation which might be formulated into a long-range program. He considered the inauguration of a periodical, comparable to the *Wheat Studies* of the Stanford Food Research Institute, in order to provide a medium for research monographs and the discipline of deadlines to be met. The formulation of a research program was still in its preliminary stages when his work suddenly ceased. Even though the Foundation was deprived of his leadership, his influence on the thought and attitude of the staff remains. His legacy was the direction and emphasis of a new program to which the details were yet to be added.

To be called an economist or an agricultural economist was always embarrassing to Alsberg. Modestly he would protest, admitting little more than that he was a former scientist turned into the field of economics. He read extensively on economic subjects, and especially on those related to his research. A good sense for problems and an analytical turn of mind gave him a high degree of competence in dealing with economic questions. He made no pretense at being steeped in the literature of economics, nor did he claim an adequate command of the techniques of economic analysis. Nevertheless his knowledge and understanding of economics were considerable. He could get to the bottom of an economic problem as well as most trained economists.

He was skeptical of a great deal of economic theory and much economic description, holding that it was hypothesis and speculation unsupported by analyzed data. He was prone to think in categories of the natural sciences and was distrustful of qualitative analysis in economics. In a 1931 address (174), reprinted below, he set forth briefly what he thought social scientists could learn from the natural scientists. "Things which cannot be measured cannot be described accurately enough for scientific analysis," he would say in urging economists to accumulate and analyze data. "I suspect that no amount of theory that does not grow out of established fact will advance the science of economics as much as the accumulation of facts, for when a body of facts has become adequate, theory to fit them comes of itself" (181). He urged a greater use of the quantitative method in the social sciences. It was a source of regret to him that time was too short for him to master the methods and statistical techniques of quantitative analysis, although he could follow the exposition with critical facility. He regularly read the statistical and mathematical journals, and became a member of the American Mathematical Society.

Alsberg found little time for his own research after he transferred to Berkeley. He completed one study of durum wheat (123) and made some progress on several unfinished manuscripts. As chairman of a special committee of the National Resources Committee, he did much to shape its comprehensive report on the resources and development of Alaska (139). But he was absorbed much of the time in administrative duties at the Giannini Foundation. Lectures, meetings, campus affairs, and extracurricular activities interrupted his other work.

Desiring to maintain his association with students, Alsberg made himself available for generous counseling. He supervised dissertations. He offered a seminar on agricultural policy. He was increasingly disturbed by the practice of social scientists of carving up problems arbitrarily into discrete subjects when the problems themselves encompassed many aspects of human behavior without respect for academic boundaries. Eventually, on invitation, and with some urging, he agreed to offer a seminar on Contemporary Social Problems—a seminar designed for undergraduates who wished to range across departmental lines. It was an experiment. He would scarcely have undertaken it if he had not devoted so much thought to interdisciplinary relations in the social sciences in the meetings of the Social Science Research Council and its Pacific Coast Regional Committee. While a better articulation of the social sciences was an objective, Alsberg's main purpose was to train students to appraise data, to analyze them discriminatingly, to draw valid conclusions, and to express themselves correctly. The seminar was popular with serious students. It would doubtless have become a regular offering if Alsberg had lived.

On the Berkeley campus, the participation of the faculty in university affairs is extensive. For those who do it effectively, committee work becomes a major responsibility. Alsberg, always a good committeeman, was soon absorbed into the committee machinery. Graduate study, international relations, social science research, promotions, and a host of other problems occupied his attention. Time-consuming though it was, committee work interested him. His judgment was good and his suggestions were wise. He knew well the art and limitations of group action. "If you want to get something done," he would say, "the thing to do is to make someone responsible for doing it." He realized the importance of prepared agenda and the value of definite proposals as a basis for deliberation. In meetings he was informal and casual but adroit. He would at times appear to ramble far from the subject, in order to bring out forcefully and effectively in the end some relevant point. He knew how to intercede. By taking a recalcitrant committeeman's side and stating for him a generous version of his views, and then by showing the basis on which agreement might be had, Alsberg could often win the recalcitrant over, maintain harmony in the group, and earn the gratitude of his confreres.

Committees were to Alsberg more than a way of reaching group decisions. They were a medium of communication, helpful if not essential to understanding and co-operation in any large organization. They were an educational device, furnishing a means of training younger men. For such reasons he believed and urged that junior as well as senior faculty members should participate in committees and conferences. It was not a matter of democracy or representation. It was a matter of apprenticeship. In urging this policy, he assisted many of his juniors to learn and contribute.

Alsberg saw in conferences an instrument of growing administrative importance about which few people were well informed. He took a keen interest in the procedures by which conferences could be made effective, and he encouraged Hobart N. Young, Secretary of the Pacific Coast Regional Committee of the Social Science Research Council, to study the problem. On more than one occasion he strained his crowded schedule to attend a conference or meeting because he could take Hobart Young along to observe the techniques employed. Characteristically, one could never be certain whether it was his own interest or Hobart's that he desired most to serve.

Alsberg's temperament and interests drew him into numerous miscellaneous activities at Stanford and California. Van Slyke has already mentioned his cordial support of the launching and publishing of the *Annual Review of Biochemistry*, his pride in its success, and his proposal of a similar venture in the social sciences. But he was reluctant himself to undertake the actual promotion of such an idea. Unless others took up his suggestions and promoted them, they often lay dormant.

At California, as at Stanford, he encouraged the faculty to participate in discussions of research problems and research findings. Because of his wide acquaintance at both Stanford and California, he became a sort of liaison officer between the two universities even before he moved to Berkeley. Afterward he continued a connection with the Food Research Institute as consultant. He served on various joint committees of the two faculties. He gave time generously to several regional committees devoted to the promotion of research.

Alsberg was always ready to promote the cause of scientific investigation. His high purpose and genuine interest drew people to him, creating an ever-expanding circle of friends and acquaintances who respected the man and the things for which he stood. To them he represented the best traditions of science and scholarship without a trace of pedantry. He was as unrelenting in fostering the application of knowledge for the betterment of human affairs as he was indefatigable in seeking it.

The students and junior associates who owed most to Alsberg can rarely unravel the complex influence he had on their intellectual development. Much of it was the contagious effect of his own curiosity and attitude. Much of it came in challenging inspirations cast up by his pointed analogies and random commentaries from a polygenetic knowledge. Much of it derived from merely observing the man in action. These students and associates recall the little incidents of large meaning, the tests of tolerance and good humor, the persuasiveness without argumentation, the widening circumference of his discussion, and the unpretentious use of a remarkable memory which he explained as his most fortunate inheritance. They recall appreciatively the advice of informed graduate students at both Stanford and California, who directed them into unexpected trails of intellectual interest with the imperative: "By all means get to know Alsberg."

Alsberg liked young people. He enjoyed their enthusiasm; he admired their curiosity; he found hope in their liberalism. In youth lay the future. He often asked to meet younger men, but took his chances on meeting the older. Discreetly he dropped the remark that he liked to associate with his juniors more than with his contemporaries. While he did not fully mean this, he did prefer an abundant admixture of youth. As time wore on, the preference seemed to grow. "I still find myself agreeing with my juniors, and even voting with them against my contemporaries," he said. He envied the opportunity which youth would have to witness things to come. Theirs was the opportunity to know more than men had ever known before.

Those who came to know Alsberg intimately enjoyed and admired his human qualities. Unhurried, even-tempered, and unexcitable, he never seemed disturbed. His self-depreciation was something of a modest pose. He was very sure of himself in many ways, yet he wanted reassurance from others. He took associates into his confidence and sought their advice. This was a method of teaching, a device for winning others over to his point of view, and simultaneously a method of checking his own conclusions. He enjoyed flattery, but he never solicited it. He used praise masterfully. "Sugar catches more flies than vinegar," he would say. Yet he was frank in recognizing the deficiencies of men, of work, and of institutions. Mistaken judgments disturbed him. Criticism depressed him. He preached to others about things he recognized to be his own weaknesses. When he was perplexed he awaited developments: "When you don't know what's going to happen, sit in the middle of the road and wait."

Alsberg's sense of humor was keen, but quiet, and not always obvious to the casual observer. Among intimate friends he joked and teased delightfully, but in other circles he seldom revealed this side of his personality. Cleverly using anecdotes, he could point up a moral in either light or serious discussion. He was fond of telling students of his professor in Germany who extravagantly spent time on long walks. When asked how, in view of his research, these walks were possible, the professor replied: "When I walk I take my mind along."

Being human, Alsberg had a share of human weaknesses. They inspired loyalty and devotion among those who worked under him. He was forgetful of appointments; he failed to get things done on time; he committed himself for more than he could possibly do. Those under him invariably came to the rescue and tried to dig him out of his embarrassments. If he reached a class, or a meeting, or a conference on time, it was often to the credit of an efficient secretary or assistant. "Heavens, where's Alsberg?" was an appropriate prelude on such occasions, as a secretary dashed out to find him. Incidents of this sort provoked amusement but no impatience. The "boss" deserved such guardianship. He never failed to tell a secretary or assistant that a job was well done if it was, or how it could be fixed up if it was not. Those who worked under him never quite realized why they would "work their heads off" for him; but that they would, and gladly, they all knew.

Alsberg cherished ideas. He enjoyed a clever phrase, but he found a clever idea exciting. He read widely and recorded notes and references, often with brief commentaries, developing a thought for further investigation. During conversations or meetings, he habitually jotted down a desired name, idea, or reference in one of the small ten-cent notebooks of which he had an unfailing supply. These notes he found to be effective aids to his memory. The mental recording, however, was typically as effective as the written.

The breadth of his reading was considerably extended by his knowledge of languages and his habit of consulting foreign sources. His command of French and German was supplemented with a reading knowledge of Dutch, Finnish, Spanish, and Italian, sufficient for scanning publications in those languages. Only his secretary had any idea of the scope of the sources consulted. She was impressed by her never-ending search for out-of-the-way articles in inaccessible publications.

As a university administrator Alsberg relied, and with good effect, on the principles he had learned in Washington. In his judgment men should be placed in positions in which they were both competent and interested. Once the right man has been appointed, he should be given clear-cut and specific responsibility and left alone to meet it. One should be careful in making promises, he urged, and equally careful in keeping them once made. Subordinates should be given their chance, pushed into the foreground, and given full credit for their accomplishments. Promotions when earned should not be delayed: "A $200-a-year raise which comes unsolicited before it is absolutely due is more effective than a $500 raise which the recipient has had to wring out of you." One must understand the emotions, the motivations, the prejudices, and the shibboleths of men. Allow full discussion and rely heavily on persuasion. Above all, be just, and in all conflicts keep your self-control, temper, and tolerance. These, among others, were the simple principles which he sought to follow in his extraordinarily successful relations with others.

"Physician, biochemist, teacher, chemist, administrator, economist—I've had a fling at all of them in turn." In these words to the graduating class at Reed College he illustrated the angular course that his career had taken in consequence of an insatiable curiosity and limited concern for wealth and fame. The angularity of his career was in no small measure the result of distractions and sidelines. But these sidelines afforded a degree of momentum which carried him along and sent him into paths that he would otherwise have missed. While his expanding extracurricular activities deprived him of time for his own research, he would be the first to wonder if perhaps they had not, from an over-all point of view, permitted him to accomplish as much as he might have done by sticking to his laboratory. But the final test was not accomplishment alone. The personal satisfactions of meeting new problems, of appeasing a roving curiosity, of marking out the fields to be plowed, of assisting others, of lending direction to intellectual effort—these were to be reckoned in the tally. He devoted his life to things which interested him. He spent it unselfishly in the quest for truth. He marshaled knowledge in the service of others. He stimulated others to pursue his leads and to emulate his methods. He would not have said confidently that he had always made the best choices in his career, but he would have been the last to say it was not a life well spent.

Social Scientist Beyond the University

By

JOHN B. CONDLIFFE*

Carl Alsberg was a highly individual personality; but he was also a member of a remarkable community group, and the product of a cosmopolitan world. The community in which he was reared had achieved a high degree of tolerance, the hallmark of civilized living.

It is difficult, in this generation, to reconstruct the civilized international world of the latter nineteenth century—a world where racial theories were confined to a lunatic fringe and national pride expressed itself in cultural rather than political terms. Travel was not harassed by passport and visa restrictions, still less by foreign-exchange controls. Values of national currencies were stable and trade had not reverted to economic warfare.

It was an age of expansion and opportunity, when the frontiers of enterprise were still wide open. America was the melting pot where there was still room for all —the cultured and the illiterate, the comfortable and the oppressed. There was as yet little sense of international strain or conflict. The nineteenth century had been one of the longest periods of freedom from great wars. Men and women could go about their personal affairs, drawing upon the resources and culture of an expanding world, without much concern for politics.

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Whatever the merits or demerits of laissez faire, it provided an environment in which the middle classes, at least, could devote their energies freely to business, science, or the arts, as their interests led them. Conversation was rarely obsessed by economic conflicts in the guise of domestic politics, and still more rarely by international crises threatening world war. There was more time for merely living. Newspapermen were not the interpreters of history, and there was less compulsion to keep up with the news, and therefore more time and energy for other pursuits.

The particular family environment in which Alsberg grew to maturity accentuated this emphasis on the arts of living. Music, science, and literature entered into the daily interests and activities of the family. There was no great consciousness of race or religion; national backgrounds were taken for granted; and politics in the shape of electoral contests were, to the growing boy, as much a part of the American panorama as baseball.

It was entirely natural, at that time, for a promising student to complete his training at one or more of the great German universities. Alsberg went to Strassburg with good German, learned naturally at home, and came away from there with good French, acquired with almost equal ease in that borderland of cultures. There was no particular reason why such a student should pay much attention to international politics. He sank quite naturally into a familiar background where he could pursue his scientific studies to better advantage. It was an easy and tolerant world because its order, national and international, seemed settled and permanent. The time had not yet come when every aspect of daily life was to be tinged with national and even racial consciousness.

To the end of his days Alsberg carried with him the confident serenity of his youth. He was a living embodiment of that quiet and almost childlike confidence which could take oneself and one's environment for granted, without self-consciousness or self-assertion. This gave him an immense advantage in the freedom and ease of mind with which he could approach international questions. Detached from emotional uncertainty, which so sadly warps personal judgments, he could adopt a scientific attitude of inquiring and patient investigation.

His gradual transition from the natural sciences through public administration to social studies reinforced this attitude. It is no longer possible, in this age of intense and minute specialization, to compass the field of human knowledge as did the universal men of the Renaissance, but Alsberg and some of his contemporaries came near to doing so. His specialist friends were constantly amazed by his store of knowledge, and still more by his mastery of it.

Many men, blessed with associative and retentive memories, are able to produce a miscellaneous collection of odd facts and ideas. Every university has such men of wide, miscellaneous knowledge, which indeed often passes for learning. But Alsberg's knowledge was not a miscellany of unrelated facts. His peculiar genius lay in a continuous questioning and arrangement of the facts he accumulated until their inner meaning was bared to him. Many students and friends can bear witness to the illumination he was able to throw on their problems by a casual reference to some other apparently remote, but clearly relevant, field of knowledge of which he was a master. Often this would be couched in an unforgettable phrase which opened up a whole new field of speculation.

Born in an age when conversation was an art to be cultivated, he loved good talk. He preferred talk to the drudgery of preparing a formal address. Though he liked to speak, the labor of preparing a speech which met his own standards was irksome, and in later life he found it difficult to spare the time for such preparation. Much of his speaking, therefore, was discursive and stimulating rather than concise. On the rare occasions when he could not escape a commitment, he would deliver a survey of the subject in hand, in which not only a vast knowledge but new approaches and ideas were casually suggested. An address on "Population" to the Honolulu conference of the Institute of Pacific Relations in 1927 (171) contained such ideas. They not only stuck in the minds of attentive hearers, but led to a whole program of research in standards of living and land utilization in which scholars from many countries participated.

Both his analytical reasoning and the economy of expression in his rather few, precisely written papers in the social sciences derived from Alsberg's training in the discipline of the natural sciences. In the time of troubles which have been with us ever since the nineteenth-century economic and political systems collapsed in the war of 1914–18, many eminent scientists have turned their attention to economic, social, and particularly international problems. There can be little doubt that Alsberg was led to his increasing interest in these questions by his experience in wartime Washington and by the chaotic disorder which followed World War I. His Commencement Address at Reed College contains a passing and characteristic allusion which betrays this fact. An increasing proportion of his time and energy was to be devoted for the rest of his life to international economic studies; but he did not rush into them.

Nowhere is his peculiar distinction more apparent than in this gradual transfer of interest. Those who work in the various fields of social studies have long been conscious that they have much to learn from the methods that have proved so fruitful in the natural sciences. Some of their number, impressed by the results, and still more perhaps by the repute, of their colleagues in the natural sciences, have aped the techniques and mannerisms of physical measurement. Others have plunged boldly into the elaboration of mathematical analyses based upon hypotheses that have not always been drawn from observed experience or directed toward the solution of issues arising from that experience. Alsberg attempted to understand and master these techniques of analysis, but he was apt to be impatient with the amateurish excesses of this method of approach, which only too readily yields a formidable new body of scholasticism. As brought out in his 1931 address (below, pages 136–41) he stood for a more truly scientific attitude built upon the gradual accumulation of tested and verified facts, rather than upon brilliant deductive reasoning from assumed hypotheses. He did not deprecate deductive reasoning, but he was aware of its dependence upon assumptions and insistent upon the importance of choosing fruitful hypotheses from which to work.

For this reason he did not enter into his new field of interest, as so many natural scientists have done, with the confidence that methods of inquiry appropriate to the experimental sciences can produce rapid and decisive results when applied to social studies. Social students have sometimes felt no little distress when men, whose eminence in the natural sciences leaves no doubt as to their capacity and discipline, have launched, with little preliminary thought, theories and prescriptions only too clearly recognizable as well-worn fallacies.

Alsberg had no illusions on this score. In a letter written in 1934 he referred incidentally to the fact that when he came to Stanford in 1921 he had to shift his field of research from the natural to the social sciences. "I knew practically no social science," he wrote. "I had to devote a good part of six or seven years to learning it. And only then was I in a position to develop a personal research program. I am now in the middle of that program. To bring it to anything like a satisfactory rounding-out will take some years."

He was always conscious of the human factor in social policy—of inertia, ignorance, prejudice, self-interest, and venality. Having come through a difficult period of public administration, he was well aware of the general "cussedness" of men in the mass, and never unduly confident of mere intellectual solutions of any social problem. This was another reason why he was cautious about, though not unsympathetic to, the neat algebraic or geometric solutions of deductive reasoning. He knew that understanding and persuasion must be achieved as a co-operative process, and that opinions, even erroneous ones, based on interest or prejudice, may be social facts of importance. In his characteristic way, he learned and taught this idea in his contacts with the Institute of Pacific Relations which gathered lay and expert opinion together in its round tables. He stressed the value of such discussion and applied it in many other organizations.

This was the point he stressed when asked to record his impressions of the work of the Commission of Inquiry on National Policy in International Economic Relations (138). The Commission was set up by the Social Science Research Council in 1933 to hold hearings in various centers before formulating a report on policy. What Alsberg got from the experience was a keener sense of the importance of opinions and of their ventilation in public hearings and discussion. He actually wrote the summary report—a clear and simple statement which is a classic piece of interpretation; but what he thought important in the Commission's work is best stated in his own words:

> When I was first appointed a member of this commission, I was completely sceptical concerning the value of the technique of holding hearings. As the work of the commission progressed, I reached quite the opposite conclusion . . . . What I had not realized was that other things besides facts are involved in the formation of judgments on economic and political questions. It is here that the hearings the commission held seemed to me invaluable. We were able to get a cross section of public sentiment which encouraged us to venture a more progressive and forward-looking set of recommendations than I think otherwise we would have had the courage to present . . . . I was profoundly impressed by the lack of any high-protectionist argument in Des Moines—the principal city of the most Republican state in the Union . . . . we found at Denver that no one had anything in particular to say about silver. We were therefore led to believe, correctly, I think, that the intermountain country is no longer really very much interested in silver; beet sugar is much more important . . . . In short, I am convinced that the hearings were

invaluable in the education of the commission and in making their thinking realistic. I think it altogether probable that, without hearings, a commission such as ours was would be very likely to make a purely academic report that might be completely sound in theory, but actually have little immediate bearing on the situation.

One of his colleagues on the Commission has written a personal appraisal of Alsberg's contribution to its work.

> His transfer to Stanford gave him opportunities for growth in making him a collaborator in the study of the social implications and national and international implications of food supply and problems of nutrition. He seized the opportunity and brought biochemistry from the laboratory into the market place.
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> This power of growth, of working on human problems in terms of science was both a proof and an extension of the flexibility of his mind. He became, within a broad but proper definition, a social scientist. In an unusual way statistics and test tubes prepared him for something that in an unaccepted way may be called statesmanship. If his work in Palo Alto and with the Giannini Foundation does not fully document this, then I will rest on the *Report of the Commission of Inquiry into National Policy in International Economic Relations* . . . .
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> . . . . the Commission [had] to find a reporter who would formulate more popularly the reasons and the evidence back of the recommendations on which the Commission had come to agreement. We put it up to Alsberg to do this job and brushed aside his modest protests. Once he undertook it he slaved at it, accepted impersonally revisions and criticisms and turned out a report that all the Commission were willing to sign then, and I, at least, am willing to support today. . . . . [It] is a sturdy, unadorned, but competent synthesis in a field that was far from biochemistry but not wholly alien to the thinking he had been doing and hearing in his work with the Food [Research] Institute. There is about this particular essay an objectivity and dispassionateness that none of the rest

of us could have attained if we had held the pen. To those with some special view it may seem colorless and uninspired. It was meant to be just that. Alsberg was a mirror who gathered light from many sources and blended and reflected it without heat. A less modest or more dogmatic mind might have given greater punch and controversial zing, but that would have been a gratification of the ego and an appeal to emotions. Alsberg did not have the first and he dealt in reason, not emotion.

His own contribution to research on international questions was made partly through the Institute of Pacific Relations, and partly through numerous personal contacts with individuals, and in international conferences and organizations—in the natural as well as the social sciences, and in government and business. It would be tedious to attempt to list these varied contacts.

Alsberg had an unusually wide range of connections bridging the natural and the social sciences, and official and unofficial institutions, in this and other countries. In his wider activities he transplanted many seeds. He came to be in a strategic position to act as a forwarding agent and liaison between varied groups. His correspondence was large, and much of it reflected his efforts to place ideas or men in productive settings. His institutional connections served the same cross-fertilizing purpose. Academic life inevitably entails a conflict between personal research and teaching, on the one hand, and time-consuming (but usually minor) administrative committee work on the other. Alsberg's easy good nature led him to perhaps too much sacrifice of his personal work; but he would defend the administrative calls upon his time. Within an institution it is a valuable service when some one smooths out personal relations, heartens the timid, counsels the rash, and encourages the younger men. Alsberg did all these admirably.

He was of course familiar with the international co-operation which, in the natural sciences, proceeds largely by brief announcements in widely circulated journals, so that workers in one country may keep abreast of what is going on in other countries. Economic, political, and social studies do not lend themselves as readily to such publication. Much of the literature is wordy. It is not easy to separate what is new from what is mere repetition or irrelevant padding, or even, since there is no specialized language, from the partial and uninformed writings of the ignorant and prejudiced. The methods of economic analysis, as compared with those of the experimental sciences, are elementary; but they require intellectual honesty, industry, and, above all, judgment. One of the greatest modern economists has described economics as "an easy subject, at which few excel." Alsberg, however, took the time to master the literature and the methods used in the social sciences. Always his main concern, while patiently sharing in the process of discussion and mutual education, was to add something to the basis of ascertained facts upon which further analysis could proceed.

When he first came, in 1927, to a conference of the Institute of Pacific Relations, the Institute was at a turning-point in its development. Begun as an instrument of good will, it was passing into a phase of more realistic discussion based upon organized research. There were, at that time, few institutions which aimed at such discussion nationally and none which attempted it by organizing comparative and collaborative studies by scholars of different national backgrounds. The Royal Institute of International Affairs in London and the

Council on Foreign Relations in New York had both emerged as a result of the need felt at the Paris Peace Conference for continuous study of the underlying facts that shape international relations. Both bodies organized individual research, study groups, and discussion; but they were distinctively national institutions, permanently housed in one center, and interested primarily in those aspects of international relations which had national importance.

The Institute of Pacific Relations attempted for the first time to stimulate research along broadly parallel lines in many countries that differed widely in national outlook and in research facilities. It was welcomed by the Oriental peoples as the first overture from the Western world which had come to them "with nothing to sell, nothing to teach, and nothing to preach." To find methods by which the high-powered Western groups might collaborate fruitfully with individuals in the Orient whose resources were scanty, but who sat upon gold mines of experience and information, was not easy.

No one was better fitted than Alsberg to suggest ways whereby this apparently unequal co-operation might steer clear of the pitfalls of academic imperialism. His attendance at a Food Conservation Conference held in Honolulu in 1924 at the request of the Pan-Pacific Union, his membership in the Social Science Research Council's Advisory Committee on International Relations in 1926–28, and his guidance of researches at Stanford financed by that Council, had given him a wide knowledge of the problems to be studied. He gained much from a journey through the Far East in June–November 1929. He was not much interested in elaborating programs of planned research, but there were many questions on which he thought it desirable to have further information, as far as possible on a comparative basis. Examples included migration and immigration restrictions, food supply, population statistics, standards and levels of living, land utilization, capital investment, and industrial production. He was content to outline fields of study in which more accurate information might in due course lead to more realistic, because better informed, discussion. Within these fields, individuals or institutions might select workable segments within their capacity and financial resources. He was content to wait till the gradual accumulation of piecemeal results could gradually be fitted together into a mosaic of proven fact.

In casual meetings and committees he put forward such notions, derived from a long experience in research organization. At a small dinner party, which was the real beginning of the research activities of the Institute of Pacific Relations, he outlined in practical detail one or two projects in the field of agricultural economics; but the methods proposed were applicable also to wider economic questions, and to legal and political issues less capable of quantitative description and analysis. The objective was to establish a more solid basis of fact for the biennial, and later triennial, discussions attended by representative groups from the various national councils of the Institute.

The particular projects—on land utilization and standards of living—which he chose to demonstrate the methods he was proposing were carried out over the years that followed, and in more recent conferences he was urging that the time had come to collate results and analyze these comparative studies. He addressed himself to the task of analyzing the studies of living standards which had been undertaken in various Pacific countries and left an unfinished manuscript, to which reference is made below.* But the influence of his ideas and methods extended over the whole range of the Institute's activities. Even before he took the responsibility of office in the Institute, he was a valued counselor of the governing committees, of the secretariat, and of the research workers scattered over Asia, Europe, Australia, and New Zealand, as well as Canada and the United States. He made opportunities for some of these research men to come to the United States for advanced work, and he was always at the disposal of any member of the Institute who sought his advice. What was true of this particular body, to which he gave so large a part of his time, was true also of many other groups with which he became associated. The range of his interests constantly widened because he was never able to resist the temptation to follow new research trails.

His greatest gift perhaps was individual counsel and encouragement. A visiting scholar or administrator, after long months of travel and consultations in remote lands, would find Alsberg's home open at Stanford, conveniently close to the first port of call. He never had anything more important to do than to give days to listening to a report of progress made, difficulties encountered, ideas in process of formulation, arrangements in course of negotiation. From his inexhaustible fund of wisdom he would probe, discuss, suggest, encourage, and send his visitor on the way equipped with introductions, but above all stimulated and heartened. Always he was generosity itself, never claiming paternity for the ideas that flowed in a steady stream to any who might use them, and always giving praise and credit, particularly to younger men. Generations of assistants and students were recipients of this generous and friendly help; but any worker in the field was welcome to it, and many whose contact with him was more remote and whose claims upon his time were slight cherish warm memories of his wise guidance at critical moments. Modest to a fault, he believed in co-operation and mutual aid. Like Chaucer's scholar, he would always "gladly learn and gladly teach."

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* The one published by-product was an address before the Royal Institute of International Affairs, London, June 24, 1937. See Number 187 in the Classified Bibliography.—Ed.

These qualities were not always evident in meetings with large groups. He was more interested in the conduct of specific researches than in methodological discussions or broad programs of research. The fact is that he was interested in the research itself, rather than in its politics, and was singularly free from any desire to hold office or to be a prominent figure. One who came to know Alsberg well at such meetings has written of his own inability to recall "personal incidents or quick flashes of incisive speech," but added:

> He had a mind fertile in ideas and broad in his concepts of their relation to other ideas and fields. He did not overwhelm you by exuding "personality" and enthusiasm. He was not, on the other hand, shy and retiring. He was mentally and physically a man of poise, justifiable self-confidence, and stability. When he undertook a task there was no flourish, but there was a carry-through in a sober sure-footed way.

The time came, however, when Alsberg felt bound to accept the responsibility of office in the Institute of Pacific Relations. As is normally the case with such bodies, a change of control became inevitable soon after its work was firmly established. The qualities necessary to pioneer a venture of this type are seldom those required for its perpetuation. Alsberg declined a pressing offer to become the executive officer of the Pacific Council, the international organ of the Institute, but he did not feel able to decline the chairmanship of the American Council's Research Committee, which carried with it membership on the International Research Committee. In 1933, he was appointed Chairman of the International Research Committee; he held that office till his death. He was simultaneously a Trustee of the American Council, Chairman of its Research Committee, and for a time Chairman of the Council itself. More than anyone else he was responsible for ultimate decisions on research policy in the American Council and in the Institute as a whole, and he was always accessible for advice on matters of detail. In 1937 he attended a meeting of the International Studies Conference at Paris as an observer from the Institute. Later he gave time to the Pacific Northwest Regional Planning Commission, applying to this narrower field lessons he had learned in international discussions.

While his major interest was always in ideas and in people with ideas, he was aware of the importance of improving the methods of conference discussion and group action. For ten years he devoted a good deal of time to the work of the Pacific Coast Regional Committee of the Social Science Research Council. In part, he was interested in the opportunities this provided for meeting his colleagues and, in particular, younger men in smaller institutions. The regional meetings also had for him the double interest that they crossed the boundary lines of academic disciplines and brought together research men who might find a focus of interest in questions arising from their common location. For much the same reasons he conducted extracurricular seminars and discussions on the Berkeley campus. But he was interested also in the technique of discussion and in methods of making conference fruitful. In this regard, he tended to be more concerned with the stimulation of individual thinking than with arriving at a consensus among a group; but this did not mean that he was content with aimless, unorganized discussion, still less with presentation of papers to which most attention is paid by their authors. He did much to further the study of processes by which programs can be carefully prepared and integrated, so that the utmost value can be drawn from the necessarily brief period of actual conference. It was in connection with the Institute of Pacific Relations that his interest in this field found its chief expression.

The officials of the Institute, and more particularly the Research Secretary, will bear witness that he drove with a loose rein. He believed in giving scope for the development of initiative and responsibility. Never feeling any need to strive for recognition or credit, he was content to advise when consulted, to encourage or to warn; but always he sought to develop the capacities of younger associates. A notable example of his preference for the ideas of younger men occurred in the Institute conference at Virginia Beach in 1939, just after the outbreak of war in Europe. A group of young men from the British Dominions pressed hard in the Research Committee, against some opposition, for the extension of studies in southeast Asia. Alsberg successfully lent his support to them, and the studies carried out in the succeeding couple of years proved invaluable to much wider circles of readers than could have been foreseen at the time. Alsberg's feeling of greater confidence in his judgment, when he agreed with his younger colleagues, was never better justified.

It would be tedious to labor the obvious. Alsberg's contribution to research and better understanding in international relations is not to be found in public appearances or statements, or even in the records of committee and council discussions. In his later years, his range of contacts widened greatly. He went to many conferences, and took an active part in local, national, and international organizations for the study of different aspects of international relations. He was no collector of offices or titles and was often, if not usually, content to listen. It was in private or group conversations that he was at his best—listening carefully to some eager young enthusiast expound the project dear to his heart, supplementing, encouraging, guiding by a phrase or word, letting in a flood of new light by some remote but revealing reference, and sending his collaborator away strengthened and encouraged; or discoursing easily from his vast knowledge in reply to some question about the distribution of plants, the composition of foods, the organization of an industry, or the history of a scientific idea, and leaving this information to be used by his hearers as they thought fit. Pericles' well-known words can be applied to him in a peculiar sense: "For the whole earth is the sepulchre of famous men and their glory is not graven only on stone above their native earth, but lives on, far away, woven into the stuff of other men's lives."

For all his rich experience and retentive memory, this store of knowledge was honestly earned by hard and careful work. Alsberg not only read omnivorously in many languages; he took careful notes and was constantly remarking cross-references and new avenues of investigation suggested by them. When he traveled he took notes of his interviews, writing them up at night. His spelling was occasionally shaky, but he was meticulous about names. These notes betray more than the wide range of his interests. Scattered between brief summaries of the information he had gleaned, about people, products, and processes, are questions to follow up, suggested by some remark made to him, by the look of the crops he had passed, or by the people in the street. He would examine the shelves of shops or the baskets of street vendors, but always with a question. To him they were evidence of economic processes, the very stuff of which international relations consist.

He published little in this field, or indeed in any field of the social sciences, and part of what he wrote is unfinished manuscript. His untimely death robbed us of much, because he had come to the point where he was ready, at last, to write on some of the most fundamental and perplexing questions that lie beneath international conflicts. He brought not only accumulated knowledge, and an insight that can only be gained by prolonged brooding over such an accumulation, but a freshness of approach that is rare indeed. The following brief extract from the unpublished manuscript, in which he had begun to explore the possibility of summarizing and analyzing the various IPR studies on living standards, is sufficient to illustrate this fact.

> The purpose of this study is to examine the repercussions upon international relations that result from the existence of disparities between different countries with respect to their several standards of living. This is a subject of not a little importance, for in the impact of man upon man disparities in standards of living are in some ways the analogue of differences of potential in the inanimate world when mass acts

upon mass or force upon force. Just as in the inorganic world differences in potential create gradients that determine the direction and intensity of flow of force or energy, so in the world of societies disparities in standards of living create gradients that determine the direction and intensity of flow of social forces. As water in moving from one level to another causes rivers to flow and turbines to spin, so the passage of individuals and peoples from one standard of living to another very largely determines the direction, the velocity, the intensity of social change. The suddenness of the fall of water, steepness of the gradient, determines whether the force set in motion is the slow, majestic flow of a river, the turbulence of a torrent tearing away at the banks that confine it, or the devastating disaster of a flood. The steepness of social gradients, the suddenness of the release of social forces such as are generated by disparities in standards of living, determine whether social changes come by slow evolution, by turbulent flow within a changing channel, or by destructive revolution.

It is significant of the mastery which Alsberg had obtained over the complexities of social questions, that this initial metaphor is followed at once by reference to psychological attitudes which influence practical policies. He did not push his metaphor beyond a suggestive analogy. Nor did he proceed, either by appeal to measurable facts or by deduction from assumed premises, to demonstrate the working out of the analogy. The usual reply of an orthodox economist, when confronted with differences in living standards, is an exposition of the law of comparative costs, or a demonstration that trade can flourish to the mutual benefit of peoples whose standards of living differ widely. Alsberg would not have denied the importance either of the theory (properly interpreted) or of the historical facts, but it is significant that he went at once to practical policies based on what are thought to be erroneous interpretations, both of theory and of facts. These policies and the psychological attitudes behind them are also facts, and in some respects the most important facts of all.

This is the most baffling aspect of the social sciences. It is the aspect which natural scientists, when they turn their trained minds to the analysis of social questions, most easily overlook. Indeed it is overlooked by many of those whose training is in the social sciences. Nothing is more typical of Alsberg's patient, truly scientific approach than the prominent place he gave to possibly erroneous opinions as among the data to be considered in his investigation—"possibly erroneous" because it may be that even those popular judgments and political attitudes, which seem to be at variance with accepted economic history and analysis, may be found to contain an element of truth. Alsberg was familiar with the fact that traditional housewifely lore, often at variance with experimental theories of nutrition, has at times been proved by more exhaustive research to embody the truths of experience. He was prepared to envisage the possibility that popular opinion in other fields might also be based in part on dimly apprehended truths outside the assumptions of rigorous theory. In any case it must be reckoned with in any realistic study.

If one characteristic adjective had to be chosen to describe Alsberg's approach to international relations, it would be "patient." "You can't hurry ideas," he would say, "but if you can talk them out, people are reasonable." This is perhaps another way of stating the impression he gave of personal serenity and detachment. He was in fact deeply concerned, even in the period of apparent stability before 1929, with the portents of impending catastrophe; but he did not fly to palliatives or nostrums. He was concerned enough to direct to the study of international disequilibria all his mental energy and accumulated experience in scientific investigation, in an effort to probe toward fundamental causes and lasting remedies.

Sometimes, toward the close of his life, he spoke a little wistfully of the fact that he had not put much on record in the form of written summaries of his conclusions, and of his fear that he might not have time to record significant results. That fear, alas, was realized; but his counsel, and the example he set, are not lost. To a very wide circle of younger men, scattered over many continents, he will always be the man who set high standards and gave positive directions to research, in what is still a new and largely uncharted field of scientific endeavor—a field which, perhaps more than any other, now calls for the best intelligence that men can summon to save themselves from the consequences of their own success in prying into the secrets of nature.

Progress in Chemistry and the Theory of Population[*][1]

By

CARL L. ALSBERG

Speculations upon the laws governing the growth of population have appeared chiefly from the pens of economists. The food supply has commonly been regarded as an important factor in limiting the increase of population, under the tacit assumption that there can be no important source of food save agriculture and the fisheries. Nevertheless, it is by no means foolish to ask the question: Can the chemist so manipulate the elements as to enhance the food supply indefinitely? Chemists and physicists alone can give the answer; yet they have rarely spoken plainly upon this subject. To consider some phases of this question is the purpose of this paper.

As a preliminary, it is necessary to review the role of food in the animal economy and also to sum up, so far as possible, present-day knowledge upon the production by artificial means of the three principal groups of foodstuffs—carbohydrates, proteins, and fats. The more important carbohydrates from the dietetic point of view are milk sugar, cane or beet sugar, starch sugar (also termed glucose), and starch. Proteins all contain nitrogen and are an important constituent of such foods as meat, milk, and eggs, in which they occur admixed with fats and, in the case of milk, admixed with a sugar as well. The fats may occur as solids or semisolids, such as suet, lard, and butter, or as liquids, such as olive and cottonseed oil.

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[*] Reprinted verbatim from *Industrial and Engineering Chemistry*, May 1924, XVI, 524–26, this paper is included for three principal reasons. The subject attracted Alsberg's interest early and late. The treatment illustrates his way of bringing scientific knowledge to bear on a large social problem. And despite gains in understanding in the twenty-five years since the paper was written, it is germane to today's discussions.—ED.

[1] Presented at the 66th Meeting of the American Chemical Society, New Haven, Conn., April 2 to 7, 1923. Received March 24, 1924.

To the adult animal, foods supply repairs and lubricants. Fuel is furnished mainly by carbohydrates, fats, and proteins; repairs, mainly by proteins and probably also by mineral salts and water. Perhaps the food accessories, substances of the vitamin type, may be regarded as playing the part of lubricants. That portion of the food supply destined for the maintenance of the animal machine in good repair is a relatively small portion of the diet. The food accessories are an extremely minute, though very important, part. They are present in such small quantities that it is quite certain they cannot be the source of appreciable amounts of energy. Whether or not the small though necessary amount of material they furnish serves to assist in keeping the organism in repair or performs some other function is not known. Certain it is that by far the greater part of the diet serves to supply energy—that is, serves as fuel for the animal machine. This is obvious on consideration of the form in which the animal organism discards or, as the physiologist would put it, excretes carbohydrates and fats. They appear in the excreta as carbon dioxide and water. But these are the very products that are formed when carbohydrates and fats are burned in a furnace. The products of combustion in a mechanical furnace and in an animal are the same. Moreover, the amount of heat formed ultimately is the same in both cases. It is in this sense that one can speak of foods as fuel, and it is this aspect of the food supply that is treated in this paper. The food accessories are not considered.

The synthesis of simple sugars and of fats presents no insuperable difficulties to the organic chemist of the present. To be sure, the synthesis of complex carbohydrates like starch and cane sugar is still beyond his power; but their synthesis is not necessary, for such carbohydrates must be converted by the digestive juices into simple sugars before man can absorb and use them. That simple sugars can be made synthetically in the laboratory—at least in their optically inactive form—is common knowledge among chemists.[2]

The situation is no different in regard to fats. That glycerol and fatty acids, the components of ordinary fats, can be made in the laboratory is also common knowledge, even though the production of neutral fats from them is not easy.[3] The fats, like the complex carbohydrates, must be split by the digestive juices before man can absorb and use them. In the case of fats this splitting takes the form of conversion into glycerol and soap.

The synthesis of proteins is not so simple, for they possess certain biological characteristics not yet reproduced in the laboratory. It is now well established, however, that large quantities of protein are not essential to the nutrition of animals, for protein nutrition is really amino-acid nutrition. It has been shown that ingested proteins are not absorbed as such. They are completely converted in the digestive tract into simple crystallizable amino acids, and these are absorbed into the blood stream.[4] These amino acids are relatively simple substances, which may be regarded as derivatives of ammonia and not very complex acids containing carbon, hydrogen, and oxygen, and in certain cases also sulfur. From the blood each tissue cell takes, from time to time, such amino acids as it needs for the synthesis of its own specific protein. Animals have been maintained upon a diet containing but little protein, the nitrogenous material consisting almost entirely of crystalline amino acids. Nearly all the amino acids from which proteins are constructed—there are probably not more than twenty-five—have already been synthesized. There is every reason to believe that before long all of them will have been so synthesized. If this proves to be the case, there is no reason to doubt that synthetic protein substitutes in the shape of amino-acid mixtures can be produced. Indeed, the science of nutrition has demonstrated clearly that only a limited number of the amino acids found in proteins are essential in the diet. It is safe, therefore, to conclude that there is every reason to believe that the three groups of foodstuffs—carbohydrates, fats, and amino acids—can or shortly will be producible by artificial means.

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[2] Bernthsen, revised by Sudborough, *A Textbook of Organic Chemistry*, 1922, p. 325. London.

[3] Hammersten and Hedin, *A Textbook of Physiological Chemistry*, 1914, p. 60. London.

The mineral salts, however necessary, need give us no concern. They are present in abundance in nature, as is, of course, that other vitally important food, water. Whether or not vitamins will ever be producible artificially, we have at present no means of surmising. Perhaps we shall always be dependent upon agriculture for them, but a system of agriculture relieved in any material measure of the necessity of producing fuel foods would be an agriculture very different from that of the present.

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[4] Mathews, *Physiological Chemistry*, 3d ed., 1920, p. 453. New York.

However, the mere fact that carbohydrates, fats, and proteins can or soon will be producible artificially cannot be taken to mean that the chemist will have it in his power to increase the food supply at will. Foodstuffs have a high fuel value. The energy that is represented by this fuel value must come from somewhere. The chemist cannot create it. To produce foodstuffs from simpler substances such as carbon dioxide, water, and nitrogen requires energy. Most of the chemical reactions involved are endothermic—that is to say, they result in the absorption or storage of energy. It is important, therefore, to inquire what quantities of energy would be required in the artificial production of the fuel-food supply of such a population as that of the United States.

The food-fuel consumption of a population is not difficult to estimate. The daily per capita consumption of the population of the United States, including men, women, and children of all classes and employments, is certainly not less than 2,700 calories. On this basis the 1920 population of the United States would require more than 100 trillion calories a year. This is the equivalent of the fuel value of more than 14 million tons of anthracite coal, about one-sixth of our ten-year average annual production. This calculation is ultraconservative, for it leads to an estimate of the net, not the gross, requirements. The gross requirement is greater, for there is considerable wastage during distribution, during preparation in the kitchen, and at the table. Total wastage is very considerable. The daily gross per capita consumption has been estimated as more than 3,500 calories.[6] Even these large quantities of energy would not be adequate for the synthesis of the country's food supply, for it cannot be assumed that the chemical reactions involved in food synthesis can be made ideally efficient. Considerable quantities of energy must be dissipated and lost in the chemical conversions. At present, the magnitude of such losses cannot be estimated. Certain it is, however, that the energy content of the products must be considerably less than the energy quantities involved in the processes.

Where is the chemist to obtain such huge quantities of energy? To present a vivid picture, the energy requirements for the synthesis of foods have been stated in terms of anthracite coal. It is startling but apparently true that, except for minor ingredients, food supplies in theory physiologically adequate to furnish energy for our present population could be obtained by the utilization of only a portion of our present production of anthracite coal; but other sources of energy might be used equally well. There is bituminous coal; there is oil; there is water power. Whether other sources, such as wind or tides, will ever be available is doubtful. Since the food synthesis problem is at bottom a fuel or energy problem, it is necessary to consider the potentialities of the various energy resources. As far as the near future is concerned, there is no doubt that energy in one of the forms mentioned above might readily be forthcoming to supplement in an important degree the available food supply. In particular, the supplies of water power are far from completely utilized. But in the larger view much less complacency is warranted.

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[6] Pearl, *The Nation's Food*, 1920, p. 245. W. B. Saunders & Co., Philadelphia.

That our present power or energy resources are narrowly limited is incontestable. That they are diminishing at a rapidly increasing rate because of increasing demand on the part of industry, of transport, and of increasing living standard, is at least extremely probable. If to these demands are added demands for the supplementing of the food supply for an ever-increasing population, it is manifest that our present sources of energy cannot be depended upon for indefinite increment to our food supply. Another source of energy will have to be found.

The energy content of our food is now derived from the sun through the agency of green plants. Agriculture, then, is just as much a fuel-producing industry as coal mining or oil well drilling. It is superior to both in that it does not drain our fuel reserves, but utilizes from day to day a small portion of the energy that streams to the earth from the sun. It is idle to speculate upon the possibility that investigations of the future may uncover or unlock new sources of energy. In the light of present knowledge, solar radiation, the same source of energy that today furnishes us our food fuel, is the only source that seems adequate to furnish important additions through the aid of synthetic food.

Agriculture is very inefficient when measured in terms of the proportion of the total solar radiation streaming upon the crop area that it renders available to man. That this must be so is obvious if we consider that, except in the tropics, most plants are dormant during the cold months of the year. They do not grow appreciably and do not store energy. Practically all the solar radiation that falls upon the crop area during this period is lost—at any rate as far as concerns its storage by the crop plant. Now energy stored in the green plant is in the form of combustible substances. Man can use but a small portion of this material. Thus in the case of wheat he uses only the seed. The straw and the roots he cannot digest and use. Indeed, of the wheat berry itself, he cannot use a considerable part, the bran. Yet straw and roots and bran enclose a portion of the solar radiation locked up by the plant. From man's point of view, the crop plant is inefficient, both because it works only a part of the year and because a portion of the energy it stores up is in a form not directly available to him as food. There are further reasons for the plant's inefficiency. The green plant is not a mere mechanism for the storage of solar radiation. It is a living organism that utilizes sunlight in order that it may live. In the process of living and maintaining itself, the plant dissipates a large part of the energy it acquires from the sun's light, and this very large quantity of energy is not recovered in the crop. Finally, the plant cannot utilize all the many kinds of light in sunlight. Those that it can use furnish not over 6.5 per cent, in theory, of the energy of the solar radiation.[6] The other waves of sunlight have not the proper amplitude of vibration to render them available to the plant. Indeed, not all the light suitable for the plant to use is actually used by it under the conditions that commonly obtain in crop production—for two reasons. One is that much of the solar radiation never impinges upon the green parts of plants; it never comes within the range of the plant's energy-storing organs. The other is that the plant can store only a part of the utilizable light rays that actually impinge upon it. The intensity of illumination is commonly much greater than the optimum. It may be reduced to nearly one-quarter that of ordinary sunlight before the rate of energy storage is changed materially.[7] The concentration in the atmosphere of carbon dioxide, which is essential to energy storage, not intensity of illumination, determines the rate of storage. The rate at which carbon dioxide can diffuse from the atmosphere into leaves to the site of energy storage is the limiting factor. Increasing the amount of carbon dioxide in the atmosphere without changing the intensity of illumination increases the rate of energy storage.[7]

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[6] Brown, *Brit. Assoc. Advancement Sci., Rept.*, Dover meeting, 1899, p. 681.

Brown and Escombe[8] have set up a very interesting "Revenue and Expenditure Account for a Leaf," which shows so clearly the relative inefficiency of green plants

REVENUE AND EXPENDITURE ACCOUNT FOR A LEAF

Revenue Account	Calories per Sq. Cm. of Leaf Lamina per Minute
Total solar radiation incident on the leaf	0.2746
Gain of energy from surroundings	0.1817
Total energy reaching 1 sq. cm. of leaf per minute	0.4563
Expenditure Account	
Energy used up in photosynthesis	0.0033
Energy used up in transpiration	0.3668
Total energy used for internal work	0.3701
Incident solar energy transmitted	0.0862
Total energy expended by 1 sq. cm. of leaf per minute	0.4563

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[7] Brown, *Nature*, 92: 525 (1905).

[8] *Proc. Roy. Soc. (London)*, 76: 85 (1905).

that it is here reproduced. The figure 0.0033 calorie, designated as the "Energy used up in photosynthesis," represents the storage of solar energy per square centimeter of leaf per minute. This is only 1.2 per cent of the total solar radiation incident per minute upon a square centimeter of leaf.

It will be noted that in this table the process of storage of energy by the green plant is termed "photosynthesis." This is the term applied to the energy-storing process by plant physiologists, because the process consists in the synthesis of sugar from carbon dioxide gas and water with the aid of light. While, for the reasons given above, the storage of energy by the green plant in a form available as food is a very inefficient process, mere photosynthesis, which makes the storage of any energy at all possible, is, as chemical reactions go, relatively very efficient. The reasons why, despite the efficiency of the process fundamental to energy storage, the net efficiency of the plant as a food producer is not greater, have been given. They may be reviewed here. Only a small portion of the sun's light is suitable for photosynthesis. Of this only a part ever reaches the photosynthetic organs of the plant. Of the energy absorbed from the solar radiation, a large part is used for the maintenance of the plant. Of that part actually stored by the plant, a large part is locked up in chemical substances that do not furnish food.

Despite the inefficiency of the green plant, the quantities of energy stored up by agriculture in such a country as the United States as food for man and fodder for domesticated animals is enormous—at least the equivalent of 10 to 20 per cent of our entire annual coal production, anthracite and bituminous combined. It must be added that a large part of the produce of agriculture is not destined for direct consumption by man, but for the feeding of domesticated animals. Straws, grass, hay, and the bulk of the corn, oat, and barley crops are fed. They are thus converted into human food and furnish meat and dairy products. This conversion is accompanied by great waste of fuel values. According to the conditions of the conversion, as little as 2 or 3 per cent or as much as 30 odd per cent of the energy value of the feed is recovered.

It is thus evident that the production of food by agriculture is, in a physical sense, inefficient. But minute portions of the total solar radiation are conserved to us in our food. It is therefore by no means hopeless for the chemist to expect ultimately to develop processes for the storage of solar radiation that will be far more efficient than the net result of agriculture. It is not necessary for him to imitate the green plant. He may find it advantageous to utilize some other portion of the sun's light than that used by the green plant. He may even devise a reaction or a series of reactions which will use a wide range of light waves of greatly different lengths instead of the few types of light waves used by the green plant. It is not necessary that these reactions reduce carbonic acid and yield sugar or starch. The separation of water into hydrogen and oxygen, of salt into sodium and chlorine, of metallic calcium from limestone would possibly do as well. It is only necessary that the reactions store solar energy in a form available for use in effecting endothermic chemical reactions.

With such stores of energy available, the chemist can produce the primary food materials even without resort to coal, oil, and water power. It will not be necessary to supplant agriculture, but rather to supplement it by relieving it of the burden of producing all the world's food calories, perhaps leaving to it the task of producing the foods that give color and flavor and tickle our fancy and our palates. Or perhaps the chemist will not need to produce foods for man, but will meet the situation by producing either feeds for conversion into meat, milk, and eggs, or nutrient materials for such microörganisms as yeast, leaving to these the elaboration of animal feeding stuffs which can be converted by the animal into human food. During the war cattle feed was produced by growing yeast in media containing ammonia and fermentable carbohydrate. Alcohol was a by-product.

Such food or feed production, if the world ever comes to it, will be a large-scale operation with all the economies and efficiencies of the large producing unit. Herein will lie a great advantage over agriculture, a small-scale, widely scattered industry with all the wastes and inefficiency that characterize small undertakings. Moreover, it is characteristic of large-scale chemical manufacturing that relatively little labor is needed. Agriculture demands an immense amount of labor per unit of product. A very large part of agricultural labor's output is necessary for the maintenance of the laborers themselves. Indeed, in many regions today agricultural labor produces but little more than is needed for its own support. To be sure, the introduction of modern methods and of modern machinery is making labor on the land more efficient and more capable of producing a surplus. Nevertheless, it is not to be expected, great as the economies of labor in agricultural production may become, that labor in agriculture can ever produce as great a surplus over and above the maintenance needs of the laborer as should be possible easily in chemical industry.

If it is true that agriculture utilizes the solar radiation so inefficiently and that labor in agriculture is so little productive, why has not the synthesis of food elements been attempted? To the physiologist and the chemist the reasons are obvious. How completely the simple food elements that the chemist already knows how to produce can be substituted for natural foods has not yet been determined adequately. It is only a few years since it has been known that amino acids may be substituted at least in great part for proteins. Not all the amino acids have as yet been synthesized. Methods for obtaining large quantities of carbonic acid must be devised. The methods of producing simple sugars and fatty acids are laboratory procedures involving many steps and many difficult operations. Probably few of the methods now in use are suitable for large-scale operations. Even if adequate quantities of cheap power were available, it would still be the work of years to translate laboratory practice into large-scale production. It is, however, very doubtful that energy from our common sources of power—coal, oil, water—is at present cheap enough to permit food synthesis to compete with agriculture. Whether the values of agricultural products and of energy from our present sources of power will ever bear such a ratio to one another as to make competition between agriculture and chemical industry feasible, it is, of course, impossible to say. It is more than probable that such competition will not be possible before the advent of some nonbiological method for the utilization of the solar radiation.

In view of the great progress in photochemical theory in the last decade, the problem is far from hopeless. No one could venture to predict when the theoretical and experimental problems will be solved. The solutions may be nearer than most men think. What is most probable is that practical application will wait on economic necessity. The work in pure science is likely to be completed before this necessity arises. The chemists' task may be done before the engineer is set to work. If the problem is not solved earlier, it will boldly confront us with the exhaustion of the world's coal and oil reserves. The solution of the world's food and fuel problem will be the same. When it is achieved, a more profound social revolution must follow than followed the invention of the steam engine, for the importance of land and agriculture will diminish; and it will remove to a far more distant horizon the bounds that now tend to limit the growth of population.

What the Social Scientist Can Learn from the Natural Scientist[*]

By

CARL L. ALSBERG

This afternoon, I propose to address myself to the question: What can the social scientist learn from the natural scientist? Since the two fields of knowledge differ primarily in subject rather than, as some would have us believe, in their essential natures, the question is not a foolish one. It might be answered in many ways, but today I will limit myself to consideration of the role of theory in the two fields, since it is in regard to theory, apparently, that practices differ most strikingly.

Scientific progress, considered historically, is not a strictly logical process, and does not proceed by syllogisms. It proceeds by two quite opposed processes—by differentiation, which furnishes new facts, and by integration, which furnishes generalizations. Let us consider the latter first.

Every generalization is a hypothesis. Hypothesis plays a necessary role in the progress of science which no one has ever contested. A new hypothesis may be based on intuition rather than on logic or proof and may be made by a great generalizing mind on quite an inadequate basis of facts. It may be nothing more than a choice between alternatives equally probable in the light of existing knowledge. When Democritus put forth the concept of the atom, some hundreds of years before the birth of Christ, he was merely making an intuitive choice between the only two possibilities conceivable: (*a*) that matter is discontinuous and not infinitely divisible; or (*b*) that it is continuous and infinitely divisible like time. Democritus had a fifty-fifty chance of being right.

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[*] This address, with another by Edward A. Ross, was given at a Symposium on the Relation of the Social Sciences to the Natural Sciences, at a meeting of the American Association for the Advancement of Science held in June 1931 at Pasadena, California. It is here reprinted, by permission, from the *Quarterly Journal of Science-Religion-Philosophy* (Los Angeles), Winter–December 1931, II, 51–56. Ross's address was given on the preceding pages, and on the following ones Charles A. Ellwood's address on "Scientific Method in Sociology" was reprinted from *Social Forces*, October 1931.—ED.

The testing of a new hypothesis is, perhaps, the most important process for which it can be used, since testing and verification lead to the discovery of new truths. The importance of a hypothesis is not that it is true, but that it can be subjected to a process of testing or verification. It matters little whether it turns out to be true or not. If it is not verified, it is because there is something unexpected or extraordinary about it. In the attempt to verify it, we come upon signposts that point the road to the discovery of something unknown and new. A rejected hypothesis is far from sterile. On the contrary, it may render as great a service as a true hypothesis, for it has given rise to experiment, observation, or reasoning that lead to new truths.

A new hypothesis is a salient in the battle front from which new territory in the unknown may be conquered. Obviously, it has the greater value the sooner it can be used, and use means testing or verification. When a hypothesis is not susceptible of early verification, it has relatively little value—indeed, may hinder progress. The atomic theory of Democritus did not stimulate research; it made no appreciable contribution to the advancement of knowledge, for it was so far in advance of the body of then-existing knowledge as to be an isolated idea. More than two thousand years were required for enough facts to accumulate to make verification possible. When, finally, the necessary body of facts became available, a new atomic theory was proposed, based on the work of Dalton, Gay-Lussac, Avogadro, and others, without any special reference to that of Democritus. Though Democritus first announced the idea, he is in no sense an ancestor of the nineteenth century theory of the atom.

If a new hypothesis has the greater value the sooner it can be tested, and if testing is dependent upon the existence of an adequate body of knowledge, then it is clear that the existence of knowledge is a condition antecedent to the formulation of verifiable hypotheses. But the accumulation of a body of facts such as may be necessary to test a hypothesis is a process of differentiation. Phenomena to be usable for integration must first be differentiated, recognized, distinguished, circumscribed, and isolated as separate and discrete facts. Upon a mass of such differentiated facts, integration can operate. Until one has a mass of discrete facts, integration is impossible. If, nevertheless, integration is attempted, if hypotheses are formulated by intuition, they are seldom soon verifiable. They have, then, little value until the time is ripe for their verification.

The circumstances that control the usefulness of a hypothesis in the natural sciences are at least equally controlling in the social sciences, for the social scientist is less fortunate than the natural scientist in that he is rarely in a position to verify or test a hypothesis by experiment. He is limited in his testing of theories by the mass of facts that happens to have been accumulated in his discipline; he cannot create new facts by experiment. He can only make new observations. He is less fortunate, furthermore, because the phenomena with which he deals are seldom the result of a single or of few causes; they are nearly always the resultants of many more factors than are involved in the phenomena with which the natural scientist deals. It is, therefore, at least as important to accumulate facts in the social as in the natural sciences.

I am convinced that in any science the accumulation of facts is of first importance. It is by the accumulation of facts that the natural sciences are progressing. When enough facts are at hand, somehow a generalizing mind always appears to arrange them according to some general principle. Usually, several such minds operate upon them at about the same time, for the same generalization is almost always made simultaneously, or nearly so, by more than one independent worker. It is often difficult to settle the resulting questions of priority. The classical example is, of course, that of Darwin and Wallace and the origin of species. When the time is ripe, because of an adequate accumulation of facts, the general unifying principle is sure to occur at about the same time to a number of persons.

The point I have been leading up to is that in the evolution of the natural sciences, hypotheses or theories advance knowledge, if they do not go beyond what it is possible to test at the time. To one who, like myself, has come into the social sciences from the natural sciences, it seems as though this were not fully realized, at least in some fields. To such a one, there seems to be too much integration, too little differentiation; too much spinning of hypotheses without regard to their verifiability, too little humble spade work in digging out facts. If, in the social sciences and especially in economics, more attention were devoted to the recording of what seem important facts and to the analysis of their significance, I am confident we should not need to worry about theory. With an adequate body of facts, theory very soon becomes obvious to any first-class mind. We would then build up a body of theory that grows out of life, instead of closed systems largely unrelated to the world of reality. We would devote less time and ingenuity to such subjects as the theory of value and the concept of marginal utility, and more time to learning how men actually behave, and what has actually happened in the economic sphere.

Social scientists would not then be divided into high-and-low-caste workers—theorists who are the high priests and all others who are but one step above the untouchables. Novices would not always aspire to begin as theorists and scorn to concern themselves with that differentiation of economic facts that is a necessary antecedent to the integration of those facts into theory. An immense amount of effort now largely wasted, would be saved, for there would be less writing of books and more writing of journal articles. Reputations in the social sciences—at least in America—are made by writing books; yet a new idea or a new fact can usually be expounded in much briefer compass. To write a book around a single idea or fact is to waste both the writer's and the reader's time. Quite otherwise is the practice in the natural sciences. Great investigators rarely write books; they are too busy investigating. Willard Gibbs, perhaps the greatest of American scientists, never wrote a book. His collected works fill but two modest volumes. The time that would be saved from writing books could be devoted to creative work. New ideas, new theories, new facts would be made known to the world more promptly and their fertilizing influence would be felt much sooner; the tempo of progress would be accelerated. If the bulk of the literature were to be found in journals, it would be possible to keep abreast of progress without having to read a whole library each year. An investigator could then feel reasonably certain that what he thought to be new was actually original. He would not run the risk of being called to task for overlooking a paragraph buried in some fat tome.

These, then, are some of the practical lessons the social scientist might learn from the natural scientist:

a) Eagerness to dig out facts, even if it is not immediately clear how they bear on theory. This implies greater modesty in the choice of objectives.

b) Less propounding of unverifiable hypotheses.

c) Less writing of books and more publication of brief communications.

*Commencement Address at Reed College**

By

CARL L. ALSBERG

When you asked me to speak to you, I was immensely flattered, but also embarrassed, for never before had I received a similar invitation. I was embarrassed because I have not sufficient faith in my own omniscience to stand up here, as is the custom of Commencement Day orators, to tell you what you ought to do with your lives, or what you should do in order that the world may be saved, or merely what you must do in order that you yourselves may be saved.

And so I was on the point of declining when President Keezer wrote me he wished I'd accept and suggested that I make this address autobiographical. Much as I wanted to oblige President Keezer, this suggestion strengthened my inclination to decline, for I don't see anything in my career important enough to hold your attention for half an hour. Also I couldn't help thinking of my nephew and niece who regard me as not a bad sort, but after all "prewar," to be listened to with

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* In introducing Dr. Alsberg on this occasion, on June 16, 1938, President Dexter Keezer said in part:

"If Dr. Alsberg, our commencement speaker by invitation of the members of the senior class, speaks in an autobiographical vein, I am responsible. When he asked me of what he might speak to good effect, I said 'of yourself and of your career.'

"I made this suggestion because Dr. Alsberg's career is one which should serve as a shining example to the graduates of a liberal college. It has been a career of remarkable breadth. . . . .

"It can be said with an extraordinary degree of truthfulness that politeness, though not with respect, for what do I know of the realities of today? Do I not prefer the prewar cigar to the postwar cigarette? They suspect, wrongly, I assure you, that I disapprove of smoking by women; and, correctly, that I have never become used to seeing women consume too much alcohol. And was I not born in New York City in the late 'seventies? Haven't I told them how one cold winter's day I searched on hands and knees through the filthy straw on the floor of a horsecar for the nickel I'd dropped out of my mittened hand, in panic lest I'd be put off by the conductor? The conductor seemed the more dangerous to a little boy in that he looked a good deal like a tramp. Streetcar conductors in those days did not wear uniforms.

My nephew and niece know I remember the dedication of Brooklyn Bridge, and Grant's funeral parade, and the election campaign between James G. Blaine and Grover Cleveland. Men who had achieved a presidential nomination were never to be spoken of simply as Blaine and Cleveland. Elderly people still called a quarter a shilling—most confusing to a little boy. My nephew and niece know that perhaps the proudest day of my life was the birthday when I was given a high-wheeled bicycle.

I went to college in New York City in the Gay 'Nineties before the College Board had been thought

Dr. Alsberg has lived the curriculum of a liberal college. And, more important, he has done so with great distinction in every field he has entered.

"Last among the long list of distinctions which is recorded under his name in *Who's Who in America* is that of being a Regent of Reed College. His vital concern with the life of our college in that capacity adds to our joy in having him speak to the members of our graduating class on this occasion."

of, when the only way to get in was to pass an entrance examination in Latin, Greek, French or German, mathematics, and other impractical subjects. The elective system was still a Harvard innovation, the subject of heated debates in academic circles. The Ph.D. labor union had not yet reached its full flowering, and one could still be a college professor without this hallmark of exposure to academic influence. So it was not strange, then, that I acquired an M.D. rather than a Ph.D. Europe, where I spent some years after the turn of the century, was still a pleasant place. What happened there interested America so little that European news rarely made the front page.

My nephew and niece have made me realize that not merely do the several nations speak different languages, but also each generation. So after all I can't blame them for their attitude that what I do and what I think is conditioned by my prewar inhibitions. And I couldn't blame you if you feel the same way. Being human, I don't feel that you would be justified, for I still find myself agreeing with my juniors, and even voting with them against my contemporaries.

I think I have made it clear why I hesitated to accept an invitation to be autobiographical. Most autobiographies are written either to tell people how clever the author is, or to curry sympathy, or as an escape. Any one of these purposes would be distasteful to me. I don't feel the need of escape or sympathy. Life has treated me better than I deserve. And I have made so many mistakes in my time that if I told the whole truth and nothing but the truth about myself no one could possibly take me for a clever fellow. And then it struck me that to learn about some of my mistakes might interest you. Perhaps it might be profitable for you if I presented myself as a horrible example of what one ought not to do. It is in this spirit that I accepted your invitation, and it is in this spirit that I shall speak.

Nothing in this life is absolute—not even wisdom. Everything one does or says or thinks in some manner reflects one's background, or, as the psychologist would put it, it is conditioned. I had an illustration of this in England last summer. I was having a look at a famous Holbein portrait of Henry the Eighth. Behind me a girl, gotten up like a Hollywood movie star and also looking at the Holbein, exclaimed, "See, mother, they have a picture of Charles Laughton here." And so, as I go on, you may well ask can I tell the difference between Henry the Eighth and a movie actor who impersonates him? There is only one way to answer such a question. It is to tell you something about my own conditioning.

Something you have already gathered from what I have told you about my prewar background. But you should also know that I never fully grew up. I am an example of arrested development, for I have never lost the curiosity that we, all of us, have as little children who drive their elders half-crazy by constantly asking them—"why?" Most of us, as we grow up, cease asking why. I didn't. And so all my life my consuming interest has been research—asking, "What is it?" "Why is it?" and "What of it?" You want to take this into account in appraising what I am about to say.

I suppose I inherited my persistent curiosity from my father who began as a university research chemist. And through him, I suppose, I also failed to acquire a proper American interest in riches. No member of my family as far back as I can go—and I can go back a long, long way—ever had much money. I suppose my lack of interest in making money was reinforced by, or perhaps due to, the fact that I, personally, never had any financial worries. I never wanted much, but I have always had enough for my wants. I never earned a penny until I was twenty-five, and had had my degree, followed by two years of post-doctoral work abroad. I then became assistant in physiological chemistry in the Harvard Medical School at the magnificent salary of $400 a year.

My insatiable curiosity, merely a symptom of my arrested development, and my indifference to financial success, have, I suppose, colored all my life and they have influenced such small store of wisdom, if it is wisdom, as I may have acquired. This, too, you should bear in mind in weighing what I am about to say to you.

And now for the horrible examples. In me you see a Jack of many trades, a master of none. I am vain enough to believe that I had the talent to become one of the two or three leading American biochemists of my generation, with an international reputation. Instead I did not stick to any one line of research long enough to become permanently identified with it. If you are not a genius—and probably none of us here today belong in this class—and if, nevertheless, you thirst for fame, stick to one thing and learn to do it supremely well. What has been so often said about the mousetrap and the path to your door is still true. Select a narrow specialty, if it's only the left hind legs of bees. Learn more about it than anyone else alive, and become the authority on the subject. Then you'll have fame of a sort.

I know this is true because it is exactly what I haven't done. My consuming curiosity has led me to push trails into strange fields of science without ever beating a single one of them into a broad highway or even a tote road. I've never resisted the temptation to start another trail into another unfamiliar field. And so I haven't become a famous scientist. I suppose I didn't have a sufficiently consuming desire to be famous. But I've had more fun. It is much more interesting to try your hand at many things, even if you don't excel at any of them, than to put on blinders and compel yourself to fix your gaze on only a few degrees of the horizon.

But I've done even worse than fail to be a specialist: I haven't even stuck to the career I started on. Physician, biochemist, teacher, chemist, administrator, economist—I've had a fling at all of them in turn. It's been fun, but it hasn't made a great man of me. Be warned by me and stick to your last, if it's fame you want or material success. But remember there's a price to be paid for this kind of success—a price not to be measured in tangible things but rather in the number of one's friends, the narrowness of one's outlook, the character of one's taste, one's capacity for understanding and enjoyment. One may in old age be bored and lonely. I recommend that you consider well the cost before you decide upon the easy career of the narrow specialist.

And this matter of sticking to your last isn't merely a matter of choosing your career and following it; it's a matter of not allowing yourself to be diverted by a thousand and one good causes, to serve on this committee, accept that office, prepare that paper, make this speech—in short, to collect a fine assortment of what Ellis Parker Butler of *Pigs Is Pigs* fame, called "goat-feathers." I commend for your consideration this essay on this subject: "Goat-feathers are the distractions, side lines, and deflections that take a man's attention from his own business and keep him from getting ahead." "Goat-feathers" look very well in *Who's Who* or in your obituary, but they interfere with your work. I know from experience, for I lack the necessary force of character to say no. It's only because for years I've had hard-boiled secretaries, both men and women, that I've been able to get anything at all done. So again be warned by me.

To have the sort of work one likes is basic for contentment. The unhappiest people I have known are those condemned to do things in which they are not interested or which they don't believe to be worth while. For people with intellectual interests, this is the source of unending wretchedness, a fact that parents, I fear, forget all too frequently. And so I am convinced that it pays in the long run—and the world will be better for it—to make large sacrifices in order that you may spend your life at the things you like rather than at the things you hate. I think I know, for it's what I've done. I've been reasonably content, but I've been far from satisfied that I've made the most of such talents as I have or such opportunities as have come to me.

In selecting your life work I recommend that you ask yourself what you want to be doing and what position you would like to hold at the age of forty-five, assuming that you were completely the captain of your fate. I have asked this question of many young men and women who have come to me for advice regarding their future. To my astonishment many of them have replied that they had never thought of their future that way—they didn't know. If any of you here today are like that, I advise you to ask yourselves these two questions: What do I want to be doing, and what do I want to be, at the age of forty-five? An honest answer to these questions will simplify immensely the question, "What next?"

Some of these young people who had never asked themselves these questions had no real intellectual interests, no intellectual curiosity. The English novelist, George Gissing, has put the situation of such people in the mouth of one of his heroes, Edwin Reardon. Reardon says, "How I envy those clerks who go by to their office in the morning! There is a day's work cut out for them. No question of mood and feeling; they have just to work *at* something, and when the evening comes, they have earned their wages, and they are free to rest and enjoy themselves." To those of you who, like Reardon, envy these clerks I am not addressing myself. I assume that you have intellectual interests and are not content with their sort of life. To you, I think, the choice of work that interests you is one of the most important factors in your happiness.

Unfortunately chance plays a large part in shaping our lives. I know it has in mine. Most of us have had to take the job we could get; not the one we would choose. This does not seem to me as serious as I used to think, because, fortunately, there are few, if any, situations that do not offer a chance. But in the words of that great Frenchman, Louis Pasteur, "Chance favors only the trained mind." You have been here at Reed for the purpose of acquiring training for your minds. There are few, if any, positions in life that do not offer a chance to such a mind. The life of Louis Pasteur is a case in point. It was a chance, an accident, that he was sent to teach at Lille, the center of French distilling. There he observed the technological troubles of the fermentation industries. They had been observed before countless thousands of times—but not by trained minds. The trained mind of Pasteur built upon these chance observations the foundation on which the sciences of zymology and bacteriology, and much else besides, are built.

Fortunately, too, there is no truly uninteresting work to a trained mind. I know some of you will challenge this statement, but I think you will find, as I have, that if you have enough intellectual curiosity to dig down to the bottom of any situation, you encounter problems fundamental enough to challenge anyone's imagination. It's more a question of one's attitude toward one's work than the nature of the work itself. The point that I wish to make in this connection is that almost any piece of work or any job is interesting and worth while, whether it be selling bonds or growing wheat or manufacturing shoes, if you regard it as a research problem instead of as a matter of routine. Any job looked at as a research problem leads inevitably to another and better job, and so on till life is done.

Many of you will ultimately be in administrative positions. I myself have had to handle a number of such jobs. I think I have learned one or two things I want to share with you. One of them is that if you have five decisions to make at any given time, it is better to make them all with reasonable promptness than to stew over them for weeks and months. It is usually better to make the decisions promptly, even if some of them are wrong, than to take a long time in order to avoid a mistake. Time is the essence of good administration. It is important that the machine be kept going. Delays and inability to get decisions ruin an organization more rapidly than a few bad decisions which can usually be rectified. You can't have a good *esprit de corps* in an organization that does not operate. Inaction is the one thing people won't stand for. People will long endure bad government, but rebel almost instantly against a government that can't govern. This is, I believe, the psychology behind much that has happened in postwar Europe.

And while I am on the subject of *esprit de corps* I want to say that nothing is more important than to see that your subordinates are given publicly the fullest credit for their achievements. Nothing sooner ruins *esprit de corps* than to have the head of an organization take credit for the work of his subordinates. To push your assistants into the foreground is only a form of enlightened selfishness. I know, for I have tried it. That's why so many of my papers have been published under the joint authorship of myself and one or more assistants. That's why all my former assistants, no matter how distinguished they may have become in the meanwhile, would gladly go to bat for me were there occasion for it. We used to say in the Department of Agriculture that an assistant's name on a publication was worth more to the *esprit de corps* of the Department than a $500 raise in his salary.

Another point for you to remember when you have subordinates whom you want to keep, men who are worth more than they are being paid, is to advance them in position or in salary before they ask for it. Don't wait until they have an opportunity to leave at a better salary. Promote them as soon as they have earned promotion. This too is a form of enlightened selfishness. A $200-a-year raise which comes unsolicited before it is absolutely due is more effective than a $500 raise which the recipient has had to wring out of you.

Try to learn to pick the right man for each job. If you can just get the right man for every position you will have little to do as an executive but play golf. But once you have picked your man, see that he has clear-cut and very definite and specific responsibility. Then leave him alone until he has demonstrated that he cannot meet the responsibility you have placed upon him. When that is obvious, displace him with another man. The way to get things done is to make each task the specific responsibility of a given individual and to give him full responsibility for and the means wherewith to do the job.

And don't forget either (I now quote Hilaire Belloc) that administration is largely based on mechanical order, whereas ruling is based on persuasion. That's why many good administrators, like the Prussians, suffer from a singular insuccess in ruling. Persuasion is the securing of the consent of the governed. It's the idea behind democracy; for democracy, it seems to me, is not a social or an economic system. It's rather a mechanism for determining what the social or economic order is to be. A democracy might conceivably be communistic if the majority voted to adopt a communistic economic system.

In any event persuasion lies at the bottom of success in governing as contrasted with administrations. The United States is fairly well-governed but very badly administered. Perhaps one is the price we have to pay for the other. If you ever have to administer an organization, I recommend that you never forget the antagonism between the two, for the responsible head of an organization cannot administer without also more or less governing. It is in governing rather than administering that boards and committees have their uses, for through representation on them the several groups that must be persuaded can be made articulate. A very important element in persuasion is to give those who are to be persuaded an opportunity to be heard, their day in court. I have only to remind you of the bitterest complaints against reciprocal trade agreements that they were made without giving those affected an opportunity to be heard. A feeling of injustice was created.

But the feeling on the part of the governed that justice is being done is a condition for success in persuasion. Hence it is important for any one in any sort of leadership job to understand the emotions, the motivations, the prejudices, and the shibboleths of those with whom he has to deal. As I look back upon my own training, I realize that one of the most valuable things I got from the study of medicine is a little knowledge of psychiatry. Time and again it has kept me from doing an injustice, by making me realize that a given personnel problem was not a matter for discipline or a court martial but rather for an alienist.

To do justice to those with whom one comes in conflict requires self-control and tolerance. Self-control, keeping your temper in a difficult situation, is an admirable technique, for you retain your objectivity, your power to reason about a situation. It gives you an immense advantage over your adversary, if he loses what you retain, self-control. Intolerance is a poor guide to correct judgment on human conduct. The inexperienced tend to be less tolerant than the mature. I would sooner be tried before an elderly than before a young judge. There is no greater justice (I now quote from J. Walter Bingham) than justice to an opponent, but it is a difficult art.

And this is I think a fitting note on which to close at this time, when there is, or so it seems to me, so little justice to opponents over much of the world.

Alsberg's Published Works

A Classified Bibliography

The items listed below are grouped as follows: (I) Scientific Papers and Notes; (II) Monographs, Books, and Contributions to Books; (III) Reports—Official and Other; (IV) Addresses, Talks, and Similar Papers; (V) Book Reviews; (VI) Miscellaneous Contributions. Though not exhaustive, the list includes a number of slight importance, chiefly in groups I, IV, and V, and a few that are available only in mimeographed form, mostly in groups III and IV. Many of Alsberg's contributions to the publications of others, especially at the Food Research Institute and the Giannini Foundation, are acknowledged by their authors in works not listed here.

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Index

Adler, Felix, 8
Adler, Herman M., 37
Adler, Isaac, 17
Adulteration, 73; *see also* Pure Food and Drugs Act
Advertising, 40–41
Advisory Committee on International Relations, Social Science Research Council, 111
Advisory Committee on Projects, 56
Agnosticism, 9
Agriculture, United States Department of, *see* Bureau of Chemistry; Bureau of Plant Industry
Agricultural chemistry, 52–54, 126, 128
Alaska, 93
Alcohol, 133, 143
Als Berg, hill in Volkmarsen, 3
Alsberg, Carl, grandfather, 3–4
Alsberg, Carl Lucas: achievements of, 13, 15, 26–32, 35–64, 74, 87, 91–92, 96, 108–9, 113, 117, 121; as administrator, 22, 35–64, 65–100, 103, 106, 109, 147, 150–53; ancestry of, 3–5; appearance of, 13, 21; as biochemist, 17–18, 20, 23–24, 27, 53, 108, 147; birth of, 3, 5; boyhood of, 3, 6, 7–17, 142–44; as chemist, 17, 122–35, 146; college career of, 15–17; at Columbia University, 15–17, 65; as committeeman, 87–88, 94–95; and conference techniques, 95, 115–16; as conversationalist 104; as Dean of Graduate Study, Stanford, 87–88, 142 n.; death of, vi; and discussion groups, 16 n.; and economics, 20–21, 27, 30, 39, 67, 73; and educational theories, 65, 85–88, 93–94, 97, 105; family heritage of, 3, 4, 5, 8–10, 34, 142–44; at Food Research Institute, ix, 20, 27–28, 63–64, 65–88, 96, 108–9, 142 n.; at Giannini Foundation, 88–100; at Harvard, 16 n., 17–19, 23–24, 25–26, 65–66, 67, 78, 142 n., 144, 147; ideas and ideals of, 13, 25–26, 27–28, 38, 41, 80–82, 85–88, 92, 99, 105, 112–13; and Institute of Pacific Relations, 75–76, 104, 106–7, 109–21; and international relations, vii, 10, 11, 61, 94, 101–21; as linguist, 7, 12, 99, 102; marriage of, 19–20; and M.D. degree, 16–17, 144, 146; natural scientist, vii–viii, 12–13, 14–17, 20–21, 23–24, 35–64, 65–100, 102, 103–6, 136–41, 147; parentage of 3–5, 14, 16–17, 146; as pharmacologist, 17–18, 20, 23; and Reed College, vii, 45–46,

100, 105, 142–53; research of, 65–66, 71–72, 73, 74, 75, 105, 109, 155–74; schooling of, 12–16; as social scientist, vii–viii, 20–21, 27, 30, 67, 74–76, 91–92, 93–94, 103–6, 110, 119–20, 135, 136–41, 147; as statistician, 73, 92–93, 106; at Stanford, vii, ix, 16 n., 20, 65–89, 89–90, 96, 108, 110, 142 n.; and taboos, boyhood, 9; traits, 10, 13, 14, 16 n., 19, 21–22, 25–26, 32, 34, 38, 46, 47, 48, 60, 65, 69–70, 72–73, 75, 76–85, 88, 89–90, 97–100, 102, 116–17, 120, 145–46; and travels abroad, 5, 17, 88, 89, 111, 115; and university, ideas of a, 85–88, 93–94, 97; as university professor, 65–100, 147; in Washington, 18, 19–20, 35–64, 65–100, 103, 106, 109, 147, 150–53; and Wendell Willkie, 11; at Woods Hole, 23, 29, 33–34; as world citizen, 10, 84, 101–2; and World War I, 45–46, 47–61, 102, 104, 105, 133; and World War II, 45–46, 151; writings of, 13, 16 n., 24, 26–31, 157–74; and young men, 32–34, 77–85, 93–94, 97, 109–10, 114, 116–17, 120, 143–53
Alsberg, Mrs. Carl Lucas, 19–20
Alsberg, Cora, 143
Alsberg, Elsa, 6, 33
Alsberg, George, 142
Alsberg, Henry, 6, 33
Alsberg, Julius, 6
Alsberg, Lucas Samuel, 3
Alsberg, Meinhard, father of Carl Lucas, 4, 5, 11, 14–15, 146
American Association for the Advancement of Science, 24, 136 n.
American Association for Cancer Research, 24
American Chemical Society, 5, 24, 122 n.
American Economic Association, 27
American Mathematical Society, 93
American Medical Association, 24
American Pharmaceutical Association, 24
American Physiological Society, 24
Amines, 30, 32, 124–34
Ammonia, 133
Angell, James R., 68, 69
Annual Review of Biochemistry, 27, 95
Anspacher Brothers, New York chemical firm, 4
Arolsen, 3
Art, 12, 13
Association of American Dairy, Food and Drug Officials, 44
Association of Official Agricultural Chemists, 44
Avogadro, Count Amadeo, 138

Baas-Becking, Lawrence, 79
Bacteria, 30
Bacteriology, 150

Index

Bailey, E. H. S., 26
Baking industry, 57–58, 74
Bamberger, Dr., principal of Workingmen's School, 12
Bancroft Literary Society, 16
Barium, 31
Baruch, Bertha, Mrs. Meinhard Alsberg, mother of Carl Lucas, 5, 14–15, 33
Baruch, Johanna, 3–4
Baruch, Meyer, the first, 3, 5
Baruch, Meyer, the second, 4, 5
Baruch, Solomon, 3, 5
Baseball, 11, 102
Bean, Donald P., director, Stanford University Press, ix
Beet sugar, 107, 123
Belloc, Hilaire, 152
Berkeley, *see* University of California
Bernard, Claude, 34
Bigelow, W. D., 37
Bingham, J. Walter, 153
Biochemistry, 17–18, 20, 23–34, 53, 108, 147
Birds' eggs, 28
Bishop of Peterborough, 9
Black, O. F., 30, 22
Blaine, James G., 143, 144
Blaine-Cleveland presidential campaign, 6–7
Board of Consulting Scientific Experts, 36
Board of Food and Drug Inspection, 36
Boston Society of Medical Sciences, 24
Bowditch Club, 16 n.
Brown, Horace T., and Escombe, 129–30
Budget system, United States, 56–57
Bunsen, Robert Wilhelm, 4
Bureau of Aircraft Production, 59
Bureau of Chemistry, v, 19–20, 24–26, 28–31, 35–64; chemists, 49; district organization, 50; field staff, 49–50; inspectors, 49–50; professional staff, 49; reorganization of, 48–52, 62–63; research, 50–51; salaries, 62, 64
Bureau of Fisheries, 23
Bureau of Plant Industry, 18, 23–24, 30, 36–37, 65–66
Butler, Ellis Parker, 147–48
By-products, 38–39

Cadmium, 31
Calkins, Robert D., ix, 65–100
Camouflage Section, United States Army, 59
Campbell, Walter G., 35 n., 52
Canning industry, 53, 71
Cannon, Walter B., 18, 26, 37
Carbohydrates, 123, 132
Carnegie Corporation of New York, viii, ix, 67–68
Carrel, Alexis, 37
Cassava, 72
Chandler, Charles F., 4–5
Chemical Abstracts, 24
Chemical biology, 23–24, 52
Chemical Foundation, 27
Chemistry, 3, 122–35
Chit-Chat Club, 16 n.
Clark, E. D., 28, 29
Clark, William Mansfield, 29, 34

Cleveland, Grover, 143, 144
Coal, 126–35
College of Physicians and Surgeons, 16–17, 65
Collegiate institutes, 7
Colloidal chemistry, 31, 68, 82
Columbia (College and) University, viii, 4–5, 12–17, 18, 43, 65
Commission of Inquiry into National Policy in International Economic Relations, 75, 107–9
Committee on Chemicals, 60
Committee on the Conservation of Tin Plate, 60
Committee of Co-operation, 60
Committee on Industrial Medicine and Surgery, 60
Committee on Nutrition, 60
Condliffe, John B., ix, 101–21
Conference techniques, 95, 115–16
Co-ordination of government agencies, 55–56
Cottonseed oil cake, 32
Council on Foreign Relations, 111
Council of National Defense, United States, 60
Crane, Winthrop Murray, 26
Crile, George W., 37
Cushing, Harvey, 37
Cyanide, 32
Cystine, 28

Dalton, John, 138
Darwin, Charles, 139
Davis, Joseph S., vii–ix, 16 n., 69–70, 71, 72, 75
Deductive reasoning, 105–6
Definitions and standards, 44
Democracy, 152
Democritus, 137–38
Department of Commerce, 55
Diastase, 30, 31
Differentiation, 136–38
Dill, D. Bruce, 79
Doolittle, R. E., 36, 37
Drugs, 40; *see also* Pure Food and Drugs Act
Durum wheat, 93
Dyes, 3, 4, 5, 17, 146

Economics, 19–20, 35–64, 70, 74, 92
Economics, agricultural, 27, 30–31, 88–92
Education, liberal, vii, 10–11, 12–17
Efficiency: of agriculture, 128–35; of chemistry, 132–35
Eggs, 28, 53
Ehrlich, Paul, 40
Eliot, Charles William, 18
Endermann and Saarbach, New York chemical firm, 16
Energy, 122–23, 126–35
Enzymes, 29–30
Ester method, 28
Ethical Culture School, 12
Ethical Culture Society, 8

Fats and oils, 71–72, 74, 123–24
Fischer, Emil, 17, 28
Fish, 28, 53
Fisher, R. A., 78–79
Flour, 58
Folin, Otto, 28, 37

Index

Food and Nutrition Division, Surgeon General's Office, 58, 60
Food Conservation Conference, 1924, at Honolulu, 111
Food and Drug Administration, 49
Food and Drugs Act, *see* Pure Food and Drugs Act
Food Research Institute, v, 20, 27–28, 63–64, 65–88, 96, 108–9
Food supply, 122–35
Frankfurter, Felix, 18, 60–61
Franklin, Benjamin, 11
Fuel, foods as, 122–23, 126–35
Fuhrman, Babette, Mrs. Meyer Baruch, Carl Lucas' maternal grandmother, 5

Garpike, 15–16
Gas masks, 59
Gas Warfare Service, 58–59
Gay-Lussac, Joseph Louis, 138
Genther, Professor at Jena, 4
Gentile and Jew, 4, 5, 7–11, 102
German-American atmosphere and ideals, 6–14, 102
Giannini Foundation, 20, 88–100, 108
Gibbon, David, 11
Gibbs, Josiah Willard, 140
Gies, William J., 37
Gissing, George, 149
Glucose, 30
Glucoside, 32
Glycerol, 124
Glycogen, 30, 31
Goethe, Johann Wolfgang von, 11
Gossypol, 32
Grains, 29; *see also* wheat
Green plants, 128–35
Growth formulae, 32, 71
Guaiac, 30

Harlem Collegiate Institute, 7
Harvard University, Medical School of, 16 n., 17–19, 23–24, 25–26, 65–66, 67, 78, 144, 147
Heine, Heinrich, 13
Hemocyanin, 29
Henderson, L. J., 18, 23
Holbein, Hans, 145
Homeopathy, 5
Hoover, Herbert, 57–58, 61, 68–69
Horseshoe crab, 29
Hotelling, Harold, 78–79
Houston, David F., 54–57
Hubbard, Henry V., 18
Humanities, 15; *see also* Liberalism
Humboldt Scientific Society, 16 n.
Hunt, Reid, 37
Hutchison, C. B., 88, 89
Huxley, Thomas H., 9
Hydrolysis, 28
Hypotheses, 136–41

Imitation, 74; *see also* Pure Food and Drugs Act
Information specialists, 41–42, 44–45
Institute of Pacific Relations, ix, 75–76, 104, 106–7, 109–21

Integration, 136–38
Interdepartmental Ammonia Committee, 59
Interdepartmental Fertilizer Committee, 59–60
International Congress of Arts and Sciences, 24
International Research Committee, Institute of Pacific Relations, 75
International Studies Conference, 115

Japan, 74
Jarinot, Principal 7
Jena, 4
Jew and Gentile, *see* Gentile and Jew
Jordan, David Starr, 66

Kaiserslautern, 5
Keezer, Dexter, vii, 142–43
Keppel, Frederick P., viii
King Olaf, ballad on, 13
Kroeber, Alfred L., viii, 3–22
Krueger, A. P., 30

"Lakes," red dyes, 4, 5
Land utilization, 91, 104, 112–13, 133, 135
Levene, P. A., 18–19, 28, 32
Liberalism, 4, 5–11, 15
Linton, Fred B., ix, 35–64
Liver, 29
Locoweed, 31
Luck, J. Murray, 27, 95
Lusk, Graham, 37
Lyman, Frank, 7

Maize, 30
M. Alsberg and Company, 5
Manual training, 12
Marine Biological Laboratory at Woods Hole, Massachusetts, 23, 24
McCall, Samuel W., 26
Melanotic tumor, 29
Mendel, Lafayette B., 37
Microbiological laboratory, 53
Molds, 30
Mount Morris School, 7
Murlin, John R., 37
Music, 10, 11, 12–14, 102

Narcotics, 40–41
National Research Council, 60
National Resources Committee, 92, 93
New York Chemical Society, 5
Nucleic acid, 28
Nutrition, 30, 74, 81; *see also* Alsberg, Carl Lucas, natural scientist

Office of Information, Department of Agriculture, 42–43
Oil, mineral, 126–35
Olive tubercle, 30
Olives, 32
Organic phosphorus compounds, 28
Osborne, Thomas B., 37
Oxygen, 29

"P. and S.," 16–17, 65
Pacific Coast Regional Committee, Social Science Research Council, 115
Pacific Northwest Regional Planning Commission, 115
Pan-Pacific Union, 111

Index

Paris green, 5
Paris Peace Conference, 111
Pasteur, Louis, 34, 149–50
Patent medicines, 40; *see also* Pure Food and Drugs Act
Patents, 54
Peebles, Emma Blount, Mrs. Carl L. Alsberg, 19–20
Pellagra, 74
Penicillium, 30
Penrose, E. F., 79
Pepsin, 34
Pharmacognosy laboratory, 53
Pharmacology, 17–18, 20, 23, 31–32, 34–64, 66
Phenolic acids, 30
Photosynthesis, 128–35
Physiological chemistry, *see* Biochemistry
Phytochemical laboratory, 53
Planetarium, 12
Poisonous Plant Laboratory, 24
Population, 73, 104, 122–35
Poultry, 53
Pratt, Joseph H., 66
Progressive education, 12
Proskauer, Joseph M., 21
Proteins, 28, 29, 53, 71, 123, 124
Psychiatry, 153
Pure Food and Drugs Act, 19, 26, 36, 38–64
Pyrocatechol, 29–30

Reardon, Edwin, 149
Redfield, Alfred C., 29
Reed College, vii, 45–46, 100, 105, 142–53
Regional laboratories, Department of Agriculture, 52–53
Rennin, 30
Research, chemical, 50–54, 62–63; *see also* Alsberg, Carl Lucas, natural scientist
Review of Economics, 27
Rice, 71
Royal Institute of International Affairs, 110–11, 113 n.

Sachs School, 7
Sahyun, Melville, 30
Salvarsan, 40
Sanitary Corps, United States Army, 58
Saponins, 31
"Saturday Morning Club," 16 n.
Schloss, Isador, 5
Schmiedeberg, Oswald, 17, 23, 28, 30
Schurz, Carl, 11
Schwartze, E. W., 32
Semitism, *see* Gentile and Jew
Sex, 9
Sherley Amendment, 40–41
Sherman, Henry C., 37
Shortening, 71–72
Silver, 107
Smith, Theobald, 37
Social Science Research Council, 75, 95, 107, 111, 115
Society of Biological Chemists, 24
Society of Pharmacology and Experimental Therapeutics, 24
Sollman, Torald, 37
Sophistication of food and drugs, 50–51, 63
Spallanzani, Lazaro, 34
Spices, 82

Standard of living, 74, 76, 104, 112–13, 118–19
Stanford University, vii, ix, 16 n., 20, 65–88, 89–90, 96, 108, 110; Advisory Board, 87–88; Graduate School of Business Administration, 82; Graduate Study Committee, 85–88; Medical School, 66; *see also* Food Research Institute
Starch, 30–31, 71, 123, 132
Stone, J. B., 30
Sugars, 123–24, 132–35; *see also* Beet sugar
Sulfur metabolism, 29
Sumner, Francis B., 23
Sun, 128–35
Surgeon General's Office, 58–59, 60
Symposium on the Relation of the Social Sciences to the Natural Sciences, 136 n.
Synthesis, 124–35

Taft, William Howard, 19, 26, 35
Tammany, 7, 12
Taylor, Alonzo E., 69–70, 76
Torah, 9
Trichloroacetic acid, 31

University of California, ix, 79, 88–100, 115; *see also* Giannini Foundation
University of Indiana, 66
University of Minnesota, 66

Van Slyke, Donald D., viii, ix, 23–34, 95
Vaughan, Victor C., 37
Virginia Beach, 116
Vitamins, 125–26
Vitellin, 28
Volkmarsen, Hessen, 3

Wallace, Alfred Russell, 139
War Industries Board, 60
War Trade Board, 58, 60
Water power, 127–35
Weber, Georges M., 71
Wharton, George Wilson, 43
Wheat, 32, 70–71, 73, 74, 75, 93; *see also* Flour
Wheat Studies, 71, 75, 92
Wickizer, V. D., 79
Wilbur, Ray Lyman, 69
Wiley, Harvey W., 19, 36, 37, 38, 41, 45
Willkie, Wendell, 11
Wilson, Edmund Beecher, 15–16
Wöhler, Friedrich, 4
Woods Hole, Massachusetts, 23, 33–34
Woodward, H. E., 30
Workingmen's School, 12, 13
World War I, 45–46, 57–61, 102, 104, 105, 133
World War II, 45–46, 151

Yeast, 133
Young, Hobart N., 95
Young, Ruth Lee (Mrs. Hobart N.) ix, 75–76, 83, 98, 99, 148

Zinc, 31
Zinsser, Hans, 8–9
Zymology, 150